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THE Psychological Monographs

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO

The Scientific Study of the College Student

BY

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The opportunity for making this application of psychological methods is due to the cooperation of Professor Leon C. Marshall, Dean of the College of Commerce and Administration of The University of Chicago. Mention of this fact is hardly necessary so patent is his connection at every point. To him the author owes more than can publicly be acknowledged. Thanks are specifically rendered for all the facilities which he unstintedly furnished for the researches of the past two years, but most gratitude is felt for his live enthusiasm and interest which have been powerful incentives at every stage of the work. At discouraging moments, when technical difficulties seemed almost unsurmountable, his unshaken confidence in the possibilities of the method and his happy combination of the common-sense and the scientific attitudes gave inspiration for renewed exertions. The author feels highly honored in being privileged to work with so intrepid an educator.

Thanks are gratefully rendered to Miss Marie Spalding of the office force of the College of Commerce and Administration who assisted throughout the entire two years. To her intelligent service is due much of the reliability which can be credited to the results.

Helpful criticism of some of the tests was contributed by Dr. Stella B. Vincent of the Chicago Normal College who used the series in several schools for Kindergarten Training.

Grateful acknowledgment is due to Dr. Harvey A. Carr of The University of Chicago for generous advice and discriminating criticism.

To Professor James R. Angell, Head of the Department of Psychology in the university, who fostered the work from its inception, the author owes much. Those who have had the privilege of working under Professor Angell know how much stimulus results from his able psychological oversight. All this

was enjoyed by this research. But in this case there was added the interest of an educator, for Professor Angell, by virtue of his position as Dean of the Faculties in the university maintained keen interest in the practical outcome of the study, and his advice from an educational point of view has thus been an additional aid.

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CHAPTER I

The Ideal of Individualized Instruction

"To-day the professor's energy is practically exhausted in his study of the subject which he is to present to the student. In the time that is coming provision must be made, either by the regular instructors or by those appointed especially for the purpose, to study in detail the man or woman to whom instruction is offered. Just as at present, in many institutions, every student upon entrance receives a careful physical examination, for the discovery of possible weaknesses, and for the provision of special corrective exercises: . . . so in the future it will be a regular function of the college to make a general diagnosis of each student.

This will be made (1) with special reference to his character—to find out whether he is responsible, or careless, or shiftless, or perhaps vicious; (2) with special reference likewise to his intellectual capacity—to discover whether he is unusually able, or bright, or average, or slow, or dull; whether he is industrious, or irregular, or lazy; (3) with reference to his special intellectual characteristics—to learn whether he is independent and original, or one who works largely along routine lines; whether his ideas are flexible or easily diverted or rigid; whether he has control of his mind or is given to mind-wandering, and to what extent he has power to overcome difficulties; (4) with reference to his special capacities and tastes—to determine whether these are evenly balanced or whether there exists a marked preference for some special subject; whether he prefers those aspects of study which are of the book type or those of a mechanical or constructive type or those of a laboratory type; whether his special gift lies along lines of an aesthetic character or those of a literary or scientific or philosophical character; whether his special aptitude, supposing it to be in the literary field, lies in criticism or interpretation or creative work . . . and finally, (5) with reference

to the social side of his nature—to judge whether he is fond of companionship; whether he is a leader or follower among his fellows; whether he is a man of affairs or devotes himself exclusively to his studies; the character of his recreations the way in which he spends his leisure hours; whether he is compelled to work for self-support or for the support of others. . . .

Such a diagnosis, when made, would serve as the basis for the selection of studies. . . .

The data thus gathered will determine the character of the advice given for the student, and of any punishment administered. . . .

This material likewise, will determine largely the career of the student. . . .

This feature of twentieth-century college education will come to be regarded as of greatest importance, and fifty years hence will prevail as widely as it is now lacking."[1]

In the decade that has elapsed since President Harper outlined the foregoing program, the demand has become even more insistent that university education be made a matter for individual adjustment. College and university authorities are awaking to a realization of the fact that they know very little about the individuals committed to their care. The student brings to the institution a preparatory record consisting of grades which represent roughly certain attainments in various academic lines. These grades, vague and unstandardized, constitute practically the only measure of the student at time of entrance. Even after four years of contact with the student, the institution has gained little by way of adequate measures of his ability. The records show a certain number of marks, which represent degrees of success in various branches of the curriculum. These markings usually indicate the number of facts the student has been able to acquire and retain until examination time. But the particular aptitudes for professional and social life which he possesses are practically undetermined. Moreover the institution lacks the power to measure the effect of its own training upon the student. It is

[1] William Rainey Harper, The Trend in Higher Education, Univ. of Chicago Press, 1905, p. 321 ff.

generally believed that academic training effects a general increase in mental power. It is assumed that four years of college discipline have a pronouncedly beneficial effect upon memory processes, reasoning ability, habits of concentration, etc. The validity of this assumption remains questioned, however, because such general developmental effect has not been objectively determined and subjected to measurement.

The rapidly evolving ideals of twentieth-century education are bringing to light another responsibility that rests upon college and university administrators. Academic failures of students must be recognized as necessary subjects for investigation. The tendency of the past has been to accept the academic "cripples" rather ungraciously as necessary phenomena of the normal curve of distribution or else summarily to dismiss them on a general charge of incompetency. An awakening conscience, however, is prompting a new attitude toward these academic weaklings—a recognition of the obligation to study the individual in order to determine the cause of his deficiencies and then to apply remedial measures. So long as an institution accepts and retains a deficient student as a matriculant it owes him not merely low grades but special efforts looking toward their elimination.

The needs of the better-than-average student are also being seen in a clearer light. Institutions of learning are coming to see that special capacity deserves special opportunity and the establishment of "honor courses" represents the recognition of the obligation to adapt instruction to individual needs.

An attempt has been made at The University of Chicago to establish a method of studying the student, and although the aim is imperfectly realized the results have shown the practicability of the ideal. The innovations have been confined to the College of Commerce and Administration where for several years Dean L. C. Marshall has been promoting a method of individualized instruction. As at present organized, it involves the following features:

1. The enrollment of the college is kept small—about 200—so that close personal relations may be maintained between student and dean.

2. Supervision of each student's course of study is insured by the regulation that the entire thirty-six undergraduate majors must be chosen with the approval of the dean.

3. The effort is made to have a long interview with each person that applies for admission to the college. The aims of this interview are (a) to make certain that the college can serve this individual, (b) to make certain that the individual understands and appreciates the requirements of the college.

4. Upon admission each student fills out (a) a life-history blank, (b) a personal record blank.

5. Inquiries are sent to high school teachers for information concerning the student.

6. Inquiries are sent to former employers for information concerning the student.

7. A printed form and a personal letter are sent to parents requesting information and cooperation.

8. The gymnasium director furnishes any significant facts which appear in the physical examination.

9. At the opening of the autumn quarter a series of freshmen conferences are conducted by the writer on the psychology and physiology of study.

10. At the end of each quarter, instructors are asked to turn in suggestions and criticisms concerning the students in their classes.

11. In addition to the close supervision over class-room work which prevails in The University of Chicago at large, advisory relations are also maintained over the non-scholastic activities of the student. At the opening of every quarter, each student fills out a blank indicating the activities and obligations he is assuming over and above the work incidental to his studies.

12. The Daily Maroon (the university newspaper) is watched and record made in the office, of the social and athletic activities of students in this college.

13. The final feature of the plan is a system of psychological examinations carried on by the writer throughout the past two years.

It is evident that the corner-stone of this plan is that the

course of each student is given individual consideration by the dean and is chosen with reference to a large mass of information he has on file. This information is from widely different sources and covers the entire range of the student's activities. It is not to be expected that all this information gives one hundred percent of reliability. Some of the replies to the inquiries, for example; give varying reports. This is to be expected. They are obviously based upon different degrees of acquaintance and they necessarily reflect different points of view. On the whole, however, it is found that a fair degree of reliability can be attached to these reports. For example, if several persons who are competent to judge, report that a student is inclined to inactivity, one has rather good grounds for concluding that the information is correct. Experience has proved these judgments reliable in so many cases that their helpfulness is quite well established. Samples of blanks are here appended showing the manner in which the data are obtained. Their purpose will be clear without further explanation.

The mental aspect of the student is a matter for psychological investigation. Therefore in 1913 a system of psychological examinations was introduced, and the remainder of this work will consist of a description of this system and a report of results.

The University of Chicago

The College of Commerce and Administration

Please state your estimate of this student and return the card to the Dean of the College of Commerce and Administration. The information will be considered confidential. It will be quite satisfactory to have this estimate stated as answers to the following questions or in any other form which you may find better adapted to the needs of the case.

Name of student ____
No. Dept. Title

Course ____

Taken ________________ Instructor ______________________________

1. In what particulars do you consider this student strong?

(OVER)

2. In what particulars do you consider this student weak?

3. Have you any other information or suggestions growing out of your experience with this student which will be helpful in shaping his curriculum?

(OVER)

The University of Chicago

The College of Commerce and Administration

This card is designed to indicate some of the qualities more frequently commented upon. It is not expected, of course, that information will be given upon all points mentioned.

Ability to grasp general principles, Ability to master details, Ability to express thoughts, Alertness, Keenness, Thoroughness, System, Open-mindedness, Initiative, Judgment, Reliability, Industry, Self-reliance, Regard for duty, Moral influence among fellows, Poise, Manner, Ability to handle people, Fondness for sports, Interest in people, Outlook on life, Popularity.

ACTIVITIES SHEET

TRY TO ANSWER EVERY QUESTION IN SPECIFIC TERMS

DO NOT WRITE ABOVE THIS LINE

Quarter__________ Name__________

How many hours per week do you expect to work in self-support this quarter?__________

In what kind of work?__________

How many hours per week did you spend in self-support your last quarter of residence?__________

In what kind of work?__________ Total remuneration for quarter__________

Underscore the statement which in your judgment best expresses the amount you contributed to your support (including college expenses) last quarter:
All——somewhere between one-half and all——some, but less than one-half——none.

In case you are returning after three or more months' absence, or are entering the University, have you been working?__________

If so, at what?__________ Rate of pay per week__________

Name of employer__________ His address__________

Have you home duties which interfere with your college work?__________ If so, how many hours per week?__________

How much time do you consume daily in getting to and from the University?__________ (In case this is negligible, do not answer the question)

In what student activities did you engage during your last quarter of residence? (Include clubs, fraternities, etc.)__________

In what student activities do you expect to engage this quarter?__________

The University of Chicago

The College of Commerce and Administration

PERSONAL RECORD

THE STUDENT WILL PLEASE FILL THIS BLANK IN HIS OWN HANDWRITING

ADDITIONAL INFORMATION ON ANY OF THE POINTS MAY BE GIVEN ON THE BACK OF THE SHEET

Date

Date and place of birth | Name

Name of parent or guardian | Permanent address

Business of parent or guardian

Businesses owned by near relatives:

NAME OF BUSINESS FIRM	PLACE	KIND OF BUSINESS	NAME OF RELATIVE

Names and addresses of high-school teachers who know you best. (Advanced standing students may use names of college teachers.)

NAME	PRESENT ADDRESS

Business or professional experience:

YEAR	WHAT	PAY	NAME OF EMPLOYER	ADDRESS OF EMPLOYER

Names and addresses of other persons who know you best:

NAME	PRESENT ADDRESS

Extent of self-support

Before coming to college | During college year | Summer vacations

Height | Weight

How is spare time spent?

Health

Number of brothers or sisters

What extra reading?

Married or single? | Family if married

Tobacco? | In what form?

Ever do any public speaking?

Drink?

Out-of-door sports

Church affiliations

Favorite amusements

Preparing for what: As definitely as you know now

DO NOT WRITE BELOW THIS LINE

CONFERENCE IMPRESSIONS

Mobility of expression | Voice

Interest in people

Dress | Articulation

Figure—slim, medium, thick-set, fat, straight, crooked, bent

Outlook on life

Manner and pose—graceful refined, vigorous, courteous, enthusiastic, winning, slow, quick, assertive, conceited, vivacious, taciturn, sleepy

In what respects inferior to fellows

Conversational ability

In what respects superior to fellows

THE UNIVERSITY OF CHICAGO

The College of Commerce and Administration

CONFIDENTIAL

____________________ has applied for admission to the College of Commerce and Administration. Inasmuch as this college aims to prepare students for actual business and professional service, it is essential for the Dean of the College to have a complete record of each student's previous business or profes - sional experience. We should accordingly appreciate your estimate of the qualities of the person mentioned above. It will be quite satisfactory to have this estimate stated as answers to some of the following questions or in any other form which you may find better suited to the needs of the case.

1. In what qualities do you consider him superior to his fellows? __________

2. In what qualities do you consider him inferior to his fellows? __________

3. Can one depend upon him? __________

4. Is he industrious? __________

5. Is he able to take correction properly? __________

6. Is his personality pleasing? __________

7. Do you know of any bad habits he possesses? __________

8. Have you any other information or suggestions growing out of your experience with this student which will be helpful in shaping his curriculum? __________

Name of person who fills out the blank __________

Official Position __________

The back of the sheet may be used for additional information.

TRAINING FOR
PUBLIC SERVICE
BUSINESS SERVICE
SECRETARIAL WORK
COMMERCIAL TEACHING
PHILANTHROPIC SERVICE

The University of Chicago

The College of Commerce and Administration

_________________________________has applied for admission to the College of Commerce and Administration. In this college, an individualized curriculum is assigned each student, the courses varying according to the past training, present capacities and contemplated occupation of the student. Obviously, accurate knowledge concerning the student is essential to the success of such a plan. We should accordingly appreciate your estimate of the qualities of the person mentioned above. It will be quite satisfactory to have this estimate stated as answers to some of the following questions or in any other form which you may find better suited to the needs of the case.

1. In what particulars is this student strong scholastically?____________________

2. In what particulars is this student weak scholastically?____________________

3. In what qualities do you consider him superior to his fellows?____________________

4. In what qualities do you consider him inferior to his fellows?____________________

5. Have you any other information or suggestions growing out of your experience with this student which will be helpful in shaping his curriculum?____________________

Name of person who fills out the blank____________________

Official position ____________________

The back of the sheet may be used for additional information.

The University of Chicago

The College of Commerce and Administration

TRAINING FOR
PUBLIC SERVICE
BUSINESS SERVICE
SECRETARIAL WORK
COMMERCIAL TEACHING
PHILANTHROPIC SERVICE

In the space below please submit in typewritten form a 200- to 250-word "life history." Make it clear-cut, concise, and business-like. Make it of such a character that the person who reads it will have a fairly clear idea of your life background. This will cause you to include statements of date and place of birth, home influences, education, travels, ambitions for the future, and any other items which have been significant in your case.

CHAPTER II

Psychological Tests for College Students

The use of psychological tests with college students is not new as the abundant literature upon Freshman tests bears witness.[2] The feature distinguishing the present application, however, is that while previous investigations have been primarily devoted to the study of tests, and rightly too, the present investigation had for its object primarily the student and his immediate relation to the university. In this case the tests were only the instrument—the student, the goal. The objection is sometimes raised that the status of mental tests is so undecided that one is not warranted in subjecting them to the arduous strain of practical application until further development is reached. To await perfection, however, is obviously impossible; furthermore, one dare not say that the patient work of past investigators has been for naught. Surely something has been gained; some difficulties have been overcome; some refinements of technique have been brought about; some pitfalls have been exposed. True, many theoretical problems remain unsolved, and in an application of this kind one encounters them at every turn. Still it seems wise to slur some of them for the sake of the goal, and while frankly admitting the tentative and incomplete nature of the results one may still ascribe some reliability to them. One may also point to the fact, now almost truistic, that theoretical psychology gains much from these early applications of psychological method to practical situations, and there is reason to hope that serious efforts to approach the actual problems of university administration by such means will serve to focus more attention upon mental tests and thereby further their development. In spite of imperfections, then, the research was undertaken with the conviction that

[2] For history of Freshman tests see Mary T. Whitely. An Empirical Study of Certain Tests for Individual Differences. Archives of Psychology. No. 19, New York. August, 1911. pp. 1-13.

psychological methods offer a mode of approach to some of the problems of university administration and a two-year trial has shown the hope to be justified. It is easily understood that in such work as this, the complete demonstration of results demands long and thorough trials by many workers, under various conditions. Therefore the claims of the present investigation must rest largely upon questions of methodology. It is in this direction that progress must first be made. The undertaking here reported was the trial of a method, and any claims for the psychological data presented are secondary to the claims adduced in favor of the method. The hypothesis adopted was that psychological measurements are helpful in making a scientific study of the student, but that when they are to be used for purposes of interpreting or controlling everyday activities, they must be accompanied by other facts of interpretative value. So in the present work, the usefulness of the tests is to be regarded as conditioned upon the other means provided for studying the student.

The general plan of the psychological examinations was to devise a system for measuring the mental capacity of college students in order to guide their college work. Secondary aims included that of measuring mental ability from time to time throughout the student's progress in order to determine the effect of college training. The way also seemed opened for comparing various groups of students in order to observe the differences among groups. The clinical aspect of the work also presented opportunity for giving individual aid to students in forming habits of study. Each of these phases will be discussed in later chapters.

Briefly stated, the plan was to choose from the large array of tests available, those that seemed most likely to be of service for diagnostic purposes. Stern[3] states the requisites of such tests as follows: "three things are evident: first,

series of tests must be arranged that will set in play the various constituent functions of intelligence; secondly, for this purpose

[3] W. Stern, Psychological Methods of Testing Intelligence, tr. Whipple, Warwick and York, Baltimore, 1914. p. 22.

there must be a wise *selection* of tests; out of the immense number of possible tests only those should be chosen that afford a decided and a reliable symptomatic value, general applicability, and possibility of objective evaluation; thirdly, there must be created a system by means of which the several particular results of the testing can be united into one *resultant value,* i.e., a value that shows the grade of intelligence of the subject objectively in an inclusive formula in which performances of different degrees of value shall in some way be compensated."

In attempting to arrange a workable series of tests it is found that there is a deplorable diversity of opinion and practice regarding particular tests, and a wide variation in manner of administering them. A further obstacle is the paucity of correlations that have been shown between specific tests and everyday activities. The ideal method of selecting tests to be used for diagnosing mental ability is to try out a large number of tests and to correlate results with a great variety of activities. The tests having the highest degree of correlation with such everyday activities would then be most reliable for diagnostic purposes. Such a method of selection is obviously beyond the reach of one investigator. Lacking the power to show such rigid correlations, however, there are other criteria that may be employed, First, it is possible to make an *a priori* selection on the basis of methodological fitness. The form in which a test shall be given may be determined to some extent by the experience of other investigators. In memory tests, in tests involving perceptual activities, in association tests, considerable work has been done, and the technique has been partially standardized. Care was taken in the present selection, to follow these standardized modes of procedure so far as possible. It was not possible to do this completely inasmuch as several complex situations were desired that required specially devised tests. Still the standardized tests were mostly used as will be observed by one familiar with the literature.

Another consideration that modifies the selection of tests for such a series is economy of time and effort. If psychological tests are to become generally useful in college and university administration they must be handled economically. This makes

group work desirable, so far as possible, and tests must be chosen that can be so adapted. In the series about to be described about half of the tests were given by group. Some of the other tests could easily be arranged for group administration. In the opinion of the writer, however, some tests should be given individually. Satisfactory diagnosis demands some personal contact with the student, and a period of time spent in the laboratory is of considerable value in enabling the psychologist to study the characteristics of the student at close range. The necessity for economy of time and effort resulted in the omission from the series of any extended learning tests. This is perhaps, unfortunate, but experience has shown that it is impracticable to commandeer the time of a large number of university students for more than three periods throughout the year.

It is also desirable, in choosing tests, to avoid those that may be seriously affected by practice. Furthermore, if the tests are to be given to the same students year after year, they should be of such a nature that memories held over from year to year will not be of assistance.

On the symptomatic side, much room for choice also exists on *a priori* grounds. It is certainly possible to select tests that call for a variety of mental activities. There exist a number of good tests for memory ability; others furnish means for measuring associative activities; almost all tests that *are* tests of intelligence demand a high degree of attention. A complete inventory of the mind is out of the question, nevertheless it is possible to select good tests for measuring fundamental psychical activities, and the net result will be an insight into the general capacity. Every test employs the whole mind, and the reaction to each one will measure the activity of the mind in one of its modes. Of course the modes are not all equal in development, but the qualitative disparities can be disregarded by the use of a kind of compensation mechanism. In the light of some of the recent investigations in the field of mental tests, there is experimental evidence for the selection of certain tests over others. Simpson,[4] after an intensive study of a variety of tests recommends the testing

[4] B. R. Simpson, Correlations of Mental Abilities, New York, 1912. p. 110.

especially of the following abilities in the relative order stated, "(a) selective thinking, (b) memory and association, (c) quickness and accuracy of perception, (d) motor control, (e) sensory discrimination." In the present collection of tests, though no attempt was made to classify rigidly, still the functions employed most actively are probably selective thinking, memory, speed of association, and quickness of perception.

Chronology of Tests and Description of Subjects

After a comprehensive survey of mental tests with the above considerations in mind, twelve were chosen for the first year's work. Manipulation of the data from these served to augment the number of measures to sixteen. Only the names of the tests will be given here; full description will follow in Chapter III.

1. Number-checking.
2. Memory for Numbers Heard.
3. Memory for Objects Seen.
4. Memory for Logical Material, Heard.
5. Secondary Memory for same.
6. Immediate Memory for Logical Material, Seen.
7. Secondary Memory for same.
8. Loss in Logical Material Heard.
9. Loss in Logical Material Seen.
10. Opposites Test.
11. Constant Increment Test.
12. Hard Directions Test, Printed.
13. Directions Test, Oral.
14. Word-building Test.
15. Sentence-building Test.
16. Business Ingenuity Test.

On February 20, 1914, the first group of students was examined with tests numbered 1, 2, 4, 6, 14 and 15, given in the above order. Two weeks later the group was called together again and tests numbered 5, 7 and 16 were given. Both group examinations took place in the morning and required one and one-half hours and one-half hour respectively. The rest of the tests were given individually at the psychological laboratory. They extended over a period of time from February 23 to March 17, 1914. They were given in the order 3, 10, 11, 12 and 13, and required about twenty minutes.

This first group of students numbered seventy-seven. It consisted of all Freshmen in the college and some others whom the dean wished examined. The figures here presented, however, are not compiled from the work of this entire group. In establishing norms for the series, it was desired to use only those records which were complete—that is, which contained a score for every test. This eliminated some records, since unavoidable absences and laboratory accidents occurred. Other records were eliminated because of language difficulty resulting from foreign birth or speech defect. As is often the case, there were also a few who misunderstood directions. Such circumstances reduced the number of usable records to forty. These forty students were classified academically as follows:

Freshmen	32	or	80%
Sophomores	6	or	19%
Juniors	2	or	1%

Thirty-one were men and nine, women. The average age October, 1913, was 19.9. The norms which are to be presented, are thus seen to be not entirely Freshmen. This will not hinder the computation of Freshman norms, however, as in all cases where comparisons are shown, the subjects are grouped academically. The results secured with this mixed group are employed only in determining relationships among the tests. This procedure seemed justified inasmuch as it was necessary to establish norms as soon as possible in order to obtain a working basis for diagnosis, and it seemed wise to retain as large a number of records as possible in order to give greater reliability to the averages. Moreover for purposes of gauging the value of the tests as a series the class of subjects used is of little account.

It should be pointed out that the confinement of this study to the College of Commerce and Administration resulted in the use of a selected group of students. In the first place these students entered the university with a high-school average grade better than 80% (according to the ruling of the university). They are further selected in that they have similar vocational aims. The college offers preparation for "careers in the practical pro-

fessions or the various branches of business, charitable and philanthropic service." These considerations undoubtedly affect the norms slightly and they were certainly influential in determining the nature of some of the tests.

The 1914 Freshmen were first examined on November 9 of that year. Forty complete records were secured from this group. Exactly the same tests were employed as were used with the previous group. The group was called together again on November 24, 1914, and tested for secondary memory as described above. Individual tests were given during the interim.

The statistical side of this investigation resolves itself into several problems. Therefore the figures will be presented from several points of view. The first task is to make an evaluation of this particular series of tests as a series. This will be assayed in Chapter IV. Chapter III will be devoted to a description of the tests.

CHAPTER III

Description and Discussion of Tests

Inasmuch as this investigation aimed primarily to make a study of the student, not of the tests, no attempt will be made to give an exhaustive critique of each test. This has already been done for some of the tests by previous investigators and standardization has already been partially accomplished. As arranged in this series, however, the tests have not been previously used, therefore they require some examination in order to ascertain their practical diagnostic value as a whole. The tests will be described in detail and the discussion will be made as full as circumstances permit.

All tests were given in the morning. All were given by the writer. When tests were given individually an assistant was present to record results and the same assistant served during both years. Before the tests were cast in final form they were used with an experimental group consisting of twelve students in psychology. This gave opportunity for the elimination of undesirable features and also served to give the experimenter facility in handling the tests.

The total series required about two and one-half hours' work from each student, the time being divided into three periods; the first group test required one and one-half hours, the second group test, one-half hour and the individual test, twenty or thirty minutes. Tests designated *g* were given by group; those marked *i* were given individually. The latter were given in the psychological laboratory, the former, in a large, well-lighted recitation room. The students were seated in alternate seats and were provided with pencils. The following remarks were made by way of introduction to the tests:

"We shall devote the next hour and a half to a series of psychological tests. You will be excused from your next class if you have one.

"I wish to quiet any fears you may entertain about these tests, by assuring you that there is nothing mysterious or occult about them. They are simply tasks such as you perform every day, involving ordinary feats of memory, reasoning and attention. The distinguishing feature is that exactly the same task is set for each person and the objective conditions are the same for

every one. I wish to obtain as true a measure as possible of your mental ability, so ask that you put forth your maximum effort.

"The results will not give a 'map' of your mind, neither will they tell your fortune as a slip from a nickel-in-the-slot machine. But we hope to obtain some facts which will assist in planning your course through the university.

"Remember, these are simply common, everyday tasks, to be performed under experimental conditions, and your only concern is to give them your entire attention and most conscientious effort."

Care was taken to avoid interruption during the tests.

Test No. 1. Number-checking[5] *g*

Materials: Forms like sample laid face downward upon the desks.[6]

Directions: "On the reverse side of the paper before you are printed the ten Arabic numerals arranged in rows like this (showing sample). When I give the signal you are to begin at the first line and go across the paper, crossing out all the sixes. Work as fast as possible for I wish to see how rapidly your mind can act; only do not miss any. The number six appears five times in each row, so you can easily tell when you have checked all in one row. Make any kind of a mark you wish. If you happen to make a mistake and cross out the wrong number, do not stop to erase—simply draw a ring around that number and I will understand. Two minutes will be given. When I call time, stop instantly and turn over the paper face-downward. I will give the signals, Turn, Go, and at the expiration of the time, Stop. Any questions?"

5 1 6 8 4 9 2 3 7 0 1 2 7 5 0 4 8 6 9 3 4 1 8 9 0 2 5 6 3 7 1 7 5 6 0 8 9 2 4 3 7 8 6 9 0 4 3 1 2 5

7 8 0 5 1 3 4 2 6 9 2 4 0 9 7 6 1 5 3 8 3 2 0 4 1 5 7 9 6 8 4 8 1 2 6 7 3 9 0 5 3 7 9 0 8 6 5 2 1 4

3 5 9 7 8 4 6 1 0 2 5 1 8 2 3 7 4 9 6 0 8 5 9 3 2 4 1 0 7 6 9 3 4 5 2 0 8 6 1 7 9 3 1 6 7 5 8 4 0 2

2 7 3 9 6 5 0 8 1 4 9 7 3 6 1 5 0 2 8 4 0 4 7 8 5 9 6 2 1 3 0 9 3 1 5 6 4 8 7 2 4 2 3 5 6 7 9 0 8 1

4 2 5 3 0 1 7 9 8 6 3 8 6 0 9 1 5 4 7 2 9 3 6 7 4 8 0 1 2 5 6 4 9 3 1 2 0 7 5 8 6 1 2 7 4 9 0 5 3 8

9 4 7 0 3 8 5 6 2 1 6 0 9 3 8 2 7 1 4 5 7 8 1 0 9 6 4 3 5 2 5 2 7 9 4 1 6 3 8 0 1 0 4 8 2 3 7 9 5 6

0 9 8 2 5 6 1 7 4 3 8 3 5 4 6 9 2 0 1 7 6 0 2 1 3 7 9 5 8 4 2 1 8 4 9 5 7 0 3 6 0 9 5 2 1 8 6 7 4 3

1 0 4 6 2 7 9 5 3 8 0 6 2 8 4 3 9 7 5 1 2 7 4 5 6 0 3 8 9 1 3 5 0 7 8 4 2 1 6 9 5 6 8 1 9 2 4 3 7 0

8 6 1 4 9 2 3 0 5 7 4 5 1 7 2 8 6 3 0 9 1 9 5 6 8 3 2 7 4 0 8 6 2 0 7 3 5 4 9 1 2 4 7 3 5 0 1 8 6 9

6 3 2 1 7 0 8 4 9 5 7 9 4 1 5 0 3 8 2 6 5 6 3 2 7 1 8 4 0 9 7 0 6 8 3 9 1 5 2 4 8 5 0 4 3 1 2 6 9 7

7 9 6 2 1 3 4 0 5 8 4 2 5 1 9 3 8 6 0 7 9 0 4 8 1 7 2 3 6 5 6 2 8 3 0 5 1 4 9 7 5 9 4 8 0 7 1 2 3 6

9 6 8 1 0 5 3 7 4 2 1 9 4 5 3 7 0 2 6 8 0 4 7 2 3 8 6 5 9 1 9 0 3 6 8 2 7 1 5 4 7 5 0 3 2 9 4 1 6 8

0 7 3 4 2 9 1 8 6 5 9 6 1 2 4 8 7 0 5 3 1 9 8 3 0 6 5 4 7 2 1 5 7 9 3 4 8 2 6 0 8 3 5 9 7 2 6 4 0 1

3 4 7 6 8 1 2 5 9 0 6 3 0 7 5 9 4 8 1 2 4 8 5 9 7 3 1 2 0 6 7 1 0 2 9 6 4 5 3 8 3 4 7 1 6 5 2 8 9 0

6 5 9 7 3 2 8 4 0 1 0 8 3 6 1 4 9 7 2 5 2 5 3 4 6 9 0 1 8 7 5 4 1 7 2 8 3 9 0 6 1 2 6 5 8 3 0 7 4 9

8 3 5 0 9 4 7 2 1 6 8 5 7 0 2 1 3 9 4 6 5 2 1 0 8 4 7 6 3 9 2 7 4 5 1 9 0 6 8 3 6 8 9 7 1 0 3 5 2 4

1 8 0 9 7 6 5 3 2 4 2 7 8 4 6 5 1 3 9 0 3 1 2 6 9 5 8 7 4 0 4 8 2 0 5 1 6 3 7 9 4 1 8 0 5 6 9 3 7 2

2 0 4 8 5 7 6 1 3 9 7 1 6 8 0 2 5 4 3 9 6 7 0 1 4 2 3 9 5 8 0 6 9 4 7 3 2 8 1 5 2 0 1 6 4 8 7 9 5 3

4 1 2 5 6 8 0 9 7 3 5 0 9 3 7 6 2 1 8 4 8 6 9 7 5 1 4 0 2 3 8 3 5 1 6 7 9 0 4 2 9 6 2 4 3 1 5 0 8 7

5 2 1 3 4 0 9 6 8 7 3 4 2 9 8 0 6 5 7 1 7 3 6 5 2 0 9 8 1 4 3 9 6 8 4 0 5 7 2 1 0 7 3 2 9 4 8 6 1 5

[5] R. S. Woodworth and F. L. Wells, Association Tests. Psychol. Rev. Mon. Sup. 1911. 13, p. 26.

[6] Blanks for Tests No. 1, 10, 11, 12, 14 and 15 were secured from C. H. Stoelting and Co. In this test and all others where time was kept, an ordinary stop-watch was used.

Method of Scoring: Each digit checked correctly counted one unit. No deductions were made for omissions or wrong figures checked.

This test, recommended by the Committee on Standardization from the American Psychological Association, involves a number of factors. Some of these are: (1) a high degree of concentration, (2) quickness of perception for visually apprehended forms, (3) speed in motor response, (4) susceptibility to eye-strain, (5) ability to persist beyond the point of fatigue. The two-minute period devoted to this work was undoubtedly one of extremely close application.

Though no errors were counted in scoring, still observation of the errors made by an individual in this test throws some light upon the care with which one does work of this nature. Of the forty individuals whose records were used, more than fifty percent (twenty-two) made no errors,

5	made	1	error,
6	"	2	errors,
3	"	3	"
2	"	5	"
1	"	8	"
1	"	11	"

As will be seen in Table IV, this test ranks lowest in degree of correlation with the results of the whole series. This would be expected in view of the fact that it places more emphasis upon motor factors than most of the other tests.

This test seems on the whole, to be a useful type of test, for the purposes for which it was used, not from the standpoint of its high correlation with "general intelligence" but for its corroborative value. It calls for the exercise of considerable speed, and in conjunction with other tests of mental quickness, probably offers some corroborative evidence in accentuating a tendency toward one extreme or the other.

The tests that are generally recognized as of a quite purely memory type will next be described. It will be noted that the

assortment used was quite varied, the attempt being made to secure rote material, material logically connected, to test memory for impressions made through two different sense avenues, and to test immediate and deferred reproduction.

Test No. 2 Numbers Heard[7] *g*

Materials: Blank sheet of paper and pencil.

Directions: "I am going to read some numbers aloud, and shall ask you to listen very carefully. When I have finished each group, and not until then, I wish you to write the numbers down just as I give them—the same numbers, in the same order. If you leave out any one, indicate its position in the group by a dash. Before giving each group I will tell you how many digits it contains. Questions?"

6135
2947

36814
57296

241637
935816

8537142
9412837

47293815
71836245

924738615
475296318

8697132504
2146073859

Practice was given with two groups of three digits each, using the numbers 816,435. Of course, the groups of four digits each also served as practice groups, inasmuch as they are too short

[7] G. M. Whipple, Manual of Mental and Physical Tests, Warwick and York, Baltimore, 1910. p. 362.

to be real tests of memory for college students. The numbers were given at the rate of two per second.

Method of Scoring:[8] The score represents the number of digits reproduced in correct order.

Test No. 3. Objects Seen. *i*

Materials: Covered box twelve by twenty by three inches, containing the following objects fastened to the bottom: fountain-pen, pencil twenty-five cent piece, envelope, ink-well, maroon ribbon, ruler, pen-filler, two-cent stamp and key.

Directions: "I am going to show you a group of objects for six seconds, then will ask you to name them aloud from memory."

Method of Scoring: The score represents the number of objects correctly reproduced.

Test No. 4. Logically Related Material, Heard *g*

Materials: Blank sheet of paper and pencil.

Directions: "I am going to read you a rather long passage and shall ask you to listen very carefully, for when I have finished, I wish you to reproduce the meaning of the passage. The passage is too long for you to remember word for word, but try to get the entire meaning, then in reproducing, use the same words as appear in the text whenever you can."

More than once, | it has happened | in the history | of science, | that a phenomenon | predicted | by theory, | has not been brought within the range of actual observation | until long afterwards.| An astronomer | predicted | the existence | and location | of the planet | Neptune, | but it was not until some time later | that the planet | was found | at the predicted spot. | Similarly, | a physicist | unfolded | theoretically | the phenomenon | of the so-called refraction of light, | but it was reserved | for a successor | to observe | the fact.| A third | instance | is found | in the fortunes | of the theory of audition.| An eminent | physiologist | of the nineteenth century | suggested | that the little | hair-cells | in

[8] For discussion of methods of scoring this test, see H. T. Woolley and C. R. Fischer, Mental and Physical Measurements of Working Children, Psychological Review Monograph Supplement, No. 77, December, 1914. p. 124 ff.

the inner | ear | vibrate | sympathetically | when appropriate | wave-lengths | reach | the ear. | Some time subsequent | to the proposal | of this explanation, | minute, | hairy | filaments | on the bodies | of Crustacea | were seen to vibrate | sympathetically | when sounds | were made | in the vicinity | of the animals.| On investigation | these hairs | were found to be connected | with the auditory nerve, | and to constitute | the mechanism of hearing | for the animal.| The analogy | of this arrangement | to the structure | of the human | inner | ear | was instantly seen, | and thus that which had previously | been propounded | as a theory | was established | as a fact.|[9]

Method of scoring: It will be noted that this passage, as does the following one, contains a main proposition and three illustrations, the last one of which is amplified. For reproduction of the main proposition two units were given; for mention of the first, second and third illustrations there were given 14, 13 and 14 units respectively. Thus by merely stating the main proposition and the illustrations, the individual could score 43. In addition to these gross divisions, the passage was further divided into 81 "ideas." Counting each of these as two-thirds of a unit, their united value is 54, which added to the 43 units mentioned, permits scoring on a basis of 97 points for correct reproduction of the passage.

Test No. 6. Logically Related Material, Seen. *g*

Materials: Papers containing the passage beginning "When a man confines—" in mimeographed form laid face-downward on the desks.

Directions: "On the reverse side of the paper before you will be found a long passage which I wish you to read carefully when I give the signal. Read it but once, then turn it over, and on the back of it write all you can recall of the passage. Be careful to read each sentence but once, then turn over the paper and reproduce the meaning as accurately as possible."

[9] Adapted from Popular Science Lectures, E. Mach. Open Court Publishing Co. Chicago, 1895. p. 29.

When a man | confines his activities | to one particular field, | attempting | to specialize therein, | there is great probability | that his capacity for enjoyments | of an aesthetic | or spiritual character | will be lost.| Let a man who loves poetry | drop all interest in literature | for a long time, | and give himself up | to the ardors | of scientific research, | and he will find | that the beauties | of poetry | in time | cease to have any charm for him.| Similarly, | let one who was deeply religious | in early life, | leave off religious activity | and turn his attention | to the pursuit of wealth, | and he will find | that the fires of religious zeal and enthusiasm | grow cold, | leaving him unresponsive | to religious appeal.| A pitiful | example | of this tendency | toward atrophy | on the part of the emotions, | is found | in the case of Darwin.| In his youth, | he was a passionate | lover of music, | but was unable to maintain his interest in it, | owing to his absorption |in scientific pursuits.| In later life,| he sought | to revive his interest | in music,| but discovered | to his intense sorrow | that he was no longer able to find enjoyment therein.| He had so long | neglected | the faculty | of musical enjoyment | that it had become completely atrophied.|

Method of Scoring: For reproduction of the main proposition, two units were given; for statement of first, second and third illustrations, 14, 13 and 14 units respectively were given. In addition to the 43 units thus credited, the passage contained 57 "ideas" which counted as one unit each, thus permitting scoring on the basis of 100.

Tests No. 5, 7, 8, 9,—Deferred Reproduction of Logically Related Material Heard and Seen.

Materials: Blank sheets of paper.

Directions: "Write all you can recall of the passage I read to you at the last psychological examination beginning. "More than once it has happened in the history of science."

"Write all you can recall of the passage you read at the last psychological examination, beginning, "When a man confines his activities to one particular field."

Method of Scoring: Same as above. It happened that in the second reproductions some subjects reproduced more "ideas"

than they did the first time. Such gains were not counted in comparing the amounts lost; the individual simply being credited with having lost nothing. To one who has scored memory tests, the difficulties of the present task are manifest. It is necessary to lay down arbitrary rules and to adhere to them closely. Some of those most constantly used in the scoring of these passages are as follows:

1. When an "idea" was repeated no credit was given for the repetition.

2. When "ideas" were interpolated which were clearly not found in the original passage, but were mere wild guesses, no credit was given.

3. In mentioning the illustrations, if an individual gave half of an illustration, half credit was given. For example, some remembered that the second illustration in the passage heard contained something about a physicist but could not recall the idea "refraction of light." Such an incomplete statement was given a credit of 7 instead of 13.

The difficulties of scoring memory passages need not be unduly magnified. It is true that the order in which the "ideas" are reproduced does not follow the original with strict fidelity, still, owing to the faithfulness of the retentive powers of the mind, it is possible, after some experience in scoring, to match up the various meanings and to identify them with the original sources with a tolerable degree of accuracy.

As already said these memory tests were planned with view to securing a variety of measures. On the basis of material they may be classed as Rote and Logical. On the basis of sense organ impressed, the material was either heard or seen. Lastly the effort was made to measure both primary and secondary memory, the latter reproduction occurrring two weeks after the first.

In manipulating the measures secured by the use of Tests 2 and 3 one is handicapped by small range of measures. This tends to obscure the meaning of the measures. In Test 2 a wider range of measures might be secured by adopting a different method of scoring. Credit might be given for position of the digits as well as for correct reproduction. This method has disadvantages,

however, some of which are discussed by Whipple[10] and Woolley and Fischer.[11]

In Test 3 the difficulty due to small range of measures might be reduced somewhat by the use of more objects exposed for a longer time. Whipple[12] suggests, however, that likelihood of such improvement is slight. This test might be arranged for administration to a group by the use of a stereopticon slide showing a number of objects. These might be exposed on a screen for a given length of time, then the subjects might write down the names of the objects. This arrangement would permit of testing for deferred memory, a difficult accomplishment when the test is given as an individual test.

In all objects tests care should be taken to use objects with simple names, and objects whose names come readily into consciousness. Otherwise the results might be misleading because it might happen that an image of the object would be before the subject, still he might fail to name it because the name would refuse to come. For this reason care was taken to select objects common to the everyday experience of university students.

The degree of logical relationship (in terms of use) between objects also affects this test. It will be noted that the objects here used are frequently associated in use, e.g., envelope, stamp, pen, etc. It is possible that with objects not so frequently associated, the results would have been different.

Reference to Table III (p. 50) shows only a slight degree of correlation between the results of these two so-called "memory tests." Their correlation with the results of the other "memory tests" is equally slight. This may be due to the small range of measures already referred to. Another reason, however, may be that these two tests involve immediate recall, which is to some extent a "restoration of the original impression which is slowly fading." The impression is still in the foreground of consciousness and partakes of much of the freshness of the original impression before it has been vitiated

[10] *Op. cit.*
[11] *Op. cit.*
[12] *Op cit.*, p. 250.

by the disturbances of time and conflicting impressions. For this reason the reproductions were probably favored by the sense avenue used in impression. In the reproduction made by persons in whom visual impressions tend to persist, the perseverance of the impression might be a factor, while in the case of persons whose visual impressions do not persist so easily, reproduction would be scanty. The same tendency applied in the auditory field might affect Test 2. Hence the memory factor, which judging by the low correlation seems to operate with unequal force if at all, may be obscured by this factor of the rapidity with which the impression fades. Most of the evidence seems to favor tests for secondary memory over those that call for immediate reproduction only.

The tests for "logical" memory were planned to show differences between immediate and deferred reproductions. Here retentive capacity is unquestionably demanded. The impressions must be retained over a considerable period of time. This results usually in some losses. The change is not only of a quantitative nature, affecting the number of "ideas" retained. It is also qualitative. The material becomes in the interim, distorted in all sorts of ways because of the entrance into the mind of conflicting impressions. Some of these become associated with those retained and become part of the fabric of the first impression. Even when new material is not introduced, the original material may lose its shape and some parts may assume prominence over others.

It should be kept in mind that the numerical results do not tell the whole story in any person's reproduction. There exist qualitative differences among the reproductions which can not be expressed numerically. Particularly is this true in deferrred reproductions. Here two persons may give the same number of "ideas," but the faithfulness with which they adhere to the original both in order and in content, may be quite different. Furthermore the same number of ideas may appear in two reproductions, but they may differ greatly in importance. This important qualitative distinction was partially provided for by the method of scoring which gave much weight to the main points of the passages. Any qualitative differences, then, con-

cerned only the minor points, and in the arrangement of the passages, it was aimed to have these minor points of approximately equal importance. In general it might be added, that subjects who have most profuse "ideas" generally give the best presentation from a qualitative standpoint, while those whose "ideas" are scanty generally give the poorest reproductions from a qualitative standpoint. On the whole, then, the above method of scoring represents the facts fairly satisfactorily.

An important factor in measuring secondary memory is the mental attitude assumed toward the time of reproduction. If a subject memorizes for the purpose of retaining only a short time, there is likelihood that he will forget when the period for which he consciously memorized has passed. On the other hand if he intends to retain for a long time, he will likely retain better than if he formed no conscious intention. Allowance for this contingency could not be made in these tests, since it was feared that if announcement were made that deferred reproduction would be required, there would be reviewing on the part of some persons. The amount of review could not be controlled, therefore it was regarded as more practicable to say nothing about future reproduction.

The attempt was made to have the two logical passages as near alike as possible. Each was constructed in the same manner, having a main proposition with three illustrations, of which the third was amplified. Each had about the same number of words. In subject-matter there is not the same equality, the material in Test No. 6 probably being easier to grasp and to retain than that in Test No. 4.

This type of test calls for attention to logical connection and so has some reasoning involved. The activity is not a mere copying of facts; the mind looks for relations and retains the facts by means of the relations apprehended. During presentation the mind is active and anticipates the next step, then reproduction is a process of reinstatement. Those persons who characteristically look for logical connection between experiences, saw at once the relation between the parts of the passages and reproduction of the main topics was an easy matter. Some of the

subjects reported that they kept track of the sub-topics on their fingers, relating each one to the main proposition. They habitually schematize experiences and place them in headings and sub-headings in their minds. Persons who do not follow connected trains of thought easily would not see the relation between the parts so clearly, therefore they would miss some sub-topics. Details, however, might be recalled in profusion regardless of propensity for straight thinking.

Judging from the degree of correlation found between the results of the memory tests it appears that the most reliable test for memory is that for deferred rather than immediate reproduction. The scores in immediate reproduction of logical material heard and seen correlate with an index of .26, but after an interval of two weeks the residuum is constant enough to produce a correlation coefficient of .54, showing that the subjects tend to assume the same positions in the group as time goes on. As might be expected, the losses correlate with a similar degree of correspondence, .54.

The retention factor in the process is probably more influential than the sense avenue utilized in making the impression. This is suggested by the low index of correlation between scores in Tests 5 and 7 and by the fact that the correlation between scores in immediate and deferred reproductions is higher, .49. Similarly that between scores in material seen and reproduced immediately and the same thing reproduced two weeks later is also higher, being .45. This conclusion is further strengthened by reference to the correlation between results of Numbers heard and Objects seen which is but .09. The results indicate that the more reliable measures of memory are obtained by the method of deferred reproduction. Of course the second reproduction was doubtless influenced somewhat by the "immediate" reproduction. This fixed the matter somewhat more firmly in mind than if immediate reproduction had not been required. It is easily seen, however, that in this preliminary investigation it was necessary to have both records for purposes of comparison.

The memory tests on the whole seem good tools with which to study the mental ability of students. Marked differences

were discovered between the two sense avenues as media of presentation for certain students, and on questioning, they reported that their experience had borne out the findings of the tests. The tests also proved to be fairly consistent measures of the efficiency of immediate versus secondary memory. These findings, also, corresponded closely with the introspections of the students. Altogether, these memory tests furnished data of value quantitatively and qualitatively, and furnish a group of measures that balance or "compensate" for those tests which emphasize particularly rapidity of the mental processes. The form in which these tests were administered makes them easy to give, but there is great need for standardization of passages suitable to college students. Many kinds of material should be studied from the standpoint of equality in difficulty, interest-value and familiarity.

Test No. 10. Opposites.[13] *i*

Materials: List of Easy and Hard Opposites.

Easy Opposites List	Hard Opposites List
long	north
soft	sour
white	out
far	weak
up	good
smooth	after
early	above
dead	sick
hot	slow
asleep	large
lost	rich
wet	dark
high	front
dirty	love
east	tall
day	open
yes	summer
wrong	new
empty	come
top	male

[13] Woodworth and Wells, *op. cit.*, p. 60.

Directions: "I am going to give you two lists of words and ask you to say the opposite to each word as quickly as possible. Do not say the word you see on the card. Give the opposite. Examples: Give opposite of *better*, of *glad*." The subject was handed each of these lists and was directed to say the opposites aloud.

Method of Scoring: The work was scored both for speed and accuracy, the former being represented by the number of seconds from the beginning of the first response to the end of the last one. In scoring accuracy, each list was graded on the basis of 100 with 5 deducted for every wrong response or failure to respond within 15 seconds. These scores were combined into a net index by dividing the time by the accuracy. The score thus represents the time required for forty reactions divided by the accuracy score. In cases where no errors were made, the score represents time alone. In such a comparatively simple test as this, it is readily apparent that accuracy is obtained with comparatively little difficulty on the part of college students. Almost half of this group of subjects obtained a score of 100 in each list. Of the forty students, 31 scored 100 in accuracy for the Easy test and 21, for the Hard test, indicating that they are rightly designated, Easy and Hard.

The scoring of Opposites tests is always made difficult by the fact that some words have more than one opposite. An arbitrary system must be adhered to in evaluating responses. The two lists here used are free from these difficulties to some extent, thanks to the work of the Committee on Standardization. Accordingly it was decided to score words only right and wrong, and not to give half-credits. In the case of a few words. however, there still remains some ambiguity. To each of the following words more than one opposite is possible, and either one was counted correct.

above	below,	under
slow	quick,	fast
front	back,	rear, behind
dead	alive,	living
open	shut,	closed
come	go,	gone, went
empty	full,	filled
far	near,	close

There are a few more words in the lists that present ambiguities, but any responses to them other than the generally accepted opposites were not counted as correct so they are not included in the above list. They offer a serious obstacle to the success of the test, however, and should be eliminated. The disadvantage of ambiguous words connects itself not only with the production of two or more words from which scoring must be made; it also concerns the mental attitude of the subject. In the opinion of the writer, these ambiguous words are productive of long pauses and incorrrect responses because of conflict of impulses. The conflict may be of a logical nature, or as Jung[14] opines, of an essentially emotional character. The process probably consists of an impulse to say one word, then before the response can be made, an impulse arises to say another word. The result is a long pause, or a complete inhibition of response, or an incorrect response. Two examples will illustrate the point. The word "come" was a stumbling-block to many of the subjects. On the theory just presented, the long pauses or failures may be due to the fact that the word has two opposites depending on the tense in which it is interpreted. In the present tense, the proper response is "go," but in the perfect tense, the opposite is "gone." "Went" was given most frequently next to the correct opposite. The word "love" was another stumbling-block. The difficulty here may be two-fold. In the first place it may consist in the fact that the word can be regared either as verb or noun. The words commonly associated with it are, "hate" and "hatred" and the conflict might produce a long pause or a complete dead-lock. There is another type of explanation, however, which instantly occurrs to one in the light of recent pronouncements from Freudian sources. It may be that the word "love" is provocative of such strong emotional reaction in these adolescents that its usually associated opposite can come into consciousness only with great difficulty. Again, there may be only an aversion to saying "hate," the pause representing a search for a milder word. It might further be questioned whether or not "hate" is the true opposite of love. The attempt was made to secure some

[14] The Association Method, A. J. of P. 21, 1910. Pp. 223 ff.

introspections on these points from those who experienced special difficulty, but without success. When asked what was in their minds at such pauses, the subjects usually replied "nothing." Of course they were untrained observers and could not throw much light upon the problem. It is not unreasonable, however, to suppose that a conflict may have been present, though the subject was not aware of its significance.

The following list, compiled from the records of 137 students taking the test for the first time during the years 1913-14 and 1914-15, shows the effect of the ambiguities. In the case of the two words "come" and "love" are included some of the incorrect responses in order to show how frequent is the tendency toward incorrect responses.

above	under 7 times
slow	quick 4 times, swift 1 time
front	behind 6 times; rear 4 times
dead	living 3 times
open	closed 8 times
come	went 14 times; gone 7 times; stay or stay away 6 times
empty	filled 1 time; vacant 1 time
far	close 6 times
love	dislike 1 time; distaste 1 time; detest 1 time; disregard 1 time; good 1 time; ill 1 time; no response 13 times.

It will be noted that the majority of these undesirable words are found in the Hard list. It is possible that its difficulties may be due to these words alone, and that when the ambiguous words in both lists are eliminated, they will be equal in difficulty.

The Opposites test, regarded as a measure of speed of mental processes, has proved acceptable in the present investigation. Reference to Table IV (p. 51) shows that this test ranks second in degree of correlation with all the tests combined. As one of a group of "speed" tests it has considerable symptomatic value. It calls for the exercise of a type of mental gymnastics quite common in everyday life, and in the experience of the ordinary person, the two ideas are so commonly coupled as to be almost automatic. Readiness of speech seems to demand such facility.

Test No. 11. Constant Increment[15] *i*

Materials: Card containing one hundred two-place numbers.

64	72	47	30
49	35	43	56
62	51	35	44
57	30	64	31
68	56	49	37
74	44	67	60
53	36	28	71
67	73	46	48
25	63	55	53
40	47	65	61
61	43	70	36
71	66	41	42
33	69	62	34
38	37	25	39
28	39	40	33
65	32	57	73
41	59	26	38
50	31	68	63
42	60	66	58
58	48	27	32
52	54	51	59
70	46	69	52
26	55	29	45
34	27	74	72
45	29	50	54

Directions: "I am going to give you a list of 100 numbers and shall ask you to add four to each number as quickly as possible, giving the sum aloud. You may practice on this list: 22, 34, 92. Begin at the top of each of the four columns and add four to each number. You need not be afraid to go fast, for the test is easy and you are not likely to make mistakes. You should be accurate, however, because every error will take off one point from your score. The main thing is to add as rapidly as possible."

Method of Scoring: Accuracy was scored by subtracting one unit for every error. The time is represented by the number of seconds required for the 100 additions. A net index was obtained by dividing the number of seconds by the accuracy score. Of the 40 students in this group, only 4 obtained perfect scores in ac-

[15] Woodworth and Wells, *op. cit.*, p. 47

curacy, but there were very few low accuracy scores, most of the errors numbering from two to five.

This test, like the preceding one, aims to measure reaction time for almost automatized response. It is easy to administer and easy to score, and when used in company with other measures of speed of mental processes, undoubtedly has some symptomatic value. However, the activity is rather highly specialized and is so patently influenced by practice that on the whole it is not recommended as desirable for a series of general utility. Its use with the present subjects was probably attended with less difficulty than with the ordinary academic group inasmuch as the members of this group had nearly all had considerable experience with adding. It is probable, however, that a less specialized type of activity would be preferable in measuring speed of mental processes. If use is made of the test in such measurements as this, the author suggests that one hundred additions is an unnecessarily large number to require. It is quite likely that twenty-five would give as useful a measure.

Test No. 12. Hard Directions[16] (Instructions printed) *i*

Materials: Blanks, like sample, laid face downward.

Directions: "On the reverse side of this paper will be found a series of directions which I wish you to carry out as rapidly as possible. The directions will require you to write certain words and to answer certain questions in spaces provided for the purpose. The object is to complete the work correctly as quickly as possible, so do not stop till you have finished."

Method of Scoring: The work was scored as to time and accuracy. Every error, of which twenty were possible, counted five off except the last direction where 2.5 was counted off for each wrong initial. A net index was secured by dividing the time by the accuracy. Fourteen of the 40 subjects scored 100 in accuracy.

With your pencil make a dot over any one of these letters, **F G H I J**, and a comma after the longest of these three words: **boy mother girl** Then, if Christmas comes in March, make a cross right here...... but if not, pass along to the next question, and tell where the sun rises............

[16] Woodworth and Wells, *op. cit.*

If you believe that Edison discovered America, cross out what you just wrote, but if it was some one else, put in a number to complete this sentence: "A horse has........feet." Write *yes,* no matter whether China is in Africa or not........; and then give a wrong answer to this question: "How many days are there in the week?" .... Write any letter except *g* just after this comma, and then write *no* if 2 times 5 are 10 .... Now, if Tuesday comes after Monday, make two crosses here........; but if not, make a circle here......or else a square here........ Be sure to make three crosses between these two names of boys: George............Henry. Notice these two numbers: 3, 5. If iron is heavier than water, write the larger number here........, but if iron is lighter write the smaller number here........ Show by a cross when the nights are longer: in summer?.......in winter?....... Give the correct answer to this question: "Does water run uphill?" .... and repeat your answer here.......... Do nothing here (5+7=........), unless you skipped the preceding question; but write the first letter of your first name and the last letter of your last name at the ends of this line:

Two difficulties are to be found with the test in its present form. It was found by experimenting with a preliminary group of subjects, that there was a tendency to misinterpret the first direction. Several persons thought they were to place a dot over the five letters whenever they appeared in the entire text. Accordingly the precaution was taken to draw a line through this direction in giving the test to the college students being studied. The test is also embarrassed by the fact that the last direction, as printed on the blanks now available is open to misinterpretation. The line at the ends of which the subject is asked to place his initials, resembles a decoration on the page. By far the greater number of subjects so regarded it, and placed their initials at the end of the printed line after the colon. Accordingly the position of the initials was disregarded in scoring.

This test seems to demand more than mere ability to follow directions. Most of the tasks are of such a nature that they can

not be performed according to the first suggestion. The impulse is checked by a conflicting or alternate command. Accordingly the activity seems to partake largely of the nature of a resistance to suggestion, or of any effort to resist impulses. In comparing the reactions of individual students to this test, it was noticeable that those who had exhibited the power to "hold their heads" in distracting circumstances stood high in this test, while those of opposite tendency were seriously disturbed. It gives opportunity to observe the student under trying circumstances, and throws considerable light upon his habits of attacking new situations.

Test No. 13. Directions. (Oral)

Materials: Two pieces of type-writer paper folded ready for insertion in a long envelope. Electric switch-board in two rooms; in the ante-room, a clock, table, book, one chair on which assistant is seated.

Directions: "I am going to give you a series of tasks which I wish you to execute as quickly and correctly as possible. There are a number of things to be held in mind, so listen to the directions very carefully."

1. Go to the room at the end of the hall and lay this paper on the chair.
2. Then hide this paper where it can not readily be found.
3. Then open switch number six.
4. Then look at the clock and see what time it says.
5. If it says it is after ten o'clock, leave the door open as you come out .
6. Bring me a book off the table.

Be as quick as possible and do not ask any questions. On his return, the subject was asked what time it was by the clock.

Method of Scoring: The accuracy with which the directions were carried out was scored on the basis of 100 by giving a credit of 16⅔ for each task. The net index was found by dividing the time by the accuracy. Only eight of the forty subjects performed the tasks exactly as directed. There is a question as to what is the fairer method of scoring. Should the basis be speed or accuracy? Though a combination of both was

adopted, some doubt was entertained as to the justice of the plan. Scores in speed and accuracy show no positive correlation (—.11). Furthermore the accuracy scores vary widely —from 50 to 100—and one would suppose that the low scores would prevent a high correlation between scores in speed and index. The correlation seems surprisingly high, however, being .95. Thus it appears that the rather low scores in accuracy did not seriously alter the standing of the individuals with respect to speed. As final evidence that the net index as thus obtained does measure speed, is the fact that scores in speed of carrying out printed directions correlate with those in the oral test by .23, and the indices of the two tests correlate with the same coefficient. Of course this still does not answer the question as to whether accuracy would not be a truer measure of this ability.

Memory plays a rather important part in the oral directions test. Success in it demands that one retain a number of details in a certain order. It also requires the ability to "hold one's head" in spite of distractions. Several distractions were introduced such as might occur in any business situation. With reference to the first direction there was only one chair in the room, and the assistant was seated on that. This disturbed many of the subjects. All were obliged to make quick decisions as to what disposition they should make of the paper. Some obeyed the instructions implicitly and asked the assistant to rise; others tucked it behind her. Still others, however, made no attempt to obey the direction and deposited the paper on the table. A further distraction was presented in making the closing of the door contingent upon the time as told by the clock on the table. The hands of the clock were always set at 10:30 and it did not run. The surprise encountered here caused some to forget the instructions about closing the door. The peculiar wording given to this direction was also slightly disconcerting. Lastly there were twelve switches in a row, and it was necessary to remember which one to throw off. The whole situation called up by this test serves to throw considerable illumination upon the way a person meets novel and complex situations.

The experience with this test shows need for several changes.

It would be better to have all the tasks performed in the same room and to dispense with the need for an assistant. The direction about the time of day might well be omitted. It was found that some subjects did not look at the clock, reporting, when they came back to the test-room, that they got the time from their watches. Of course this counted against the score, constituting a deviation from the directions, but it should be omitted from the test because it offers opportunity for misinterpretation.

Test No. 14. Word-building[17] *g*

Materials: Blank sheet of paper with capital letters, B, M, T, A, E, O across the top laid face-downward upon the desk.

Directions: "On the reverse side of the paper before you are six letters which I wish you to use in building words. Make as many words as possible from the six letters. For example, out of the letters e, a, r, i, l, p, you might form words like rap, lip, etc. You may use any number of letters from one to six, but no other letters than these six are to be used. Any English words will do—proper names, interjections: only be sure that they fulfill the above conditions. Five minutes will be given."

Test No. 15. Sentence-building.[18] *g*

Materials: Blank paper and pencil.

Directions: "I will give you five minutes in which to make as many sentences as possible containing three words which I will give you presently. For example, if I gave you the words *money, river, Chicago,* you might make a sentence like this: "Chicago spends much money improving its river." You may use either singular or plural forms of the words, nominative, objective or possessive case. Simply use all three of the words in a sensible sentence and make as many different sentences as possible. The three words are *citizen, horse, decree.*"

Method of Scoring: The score represents the number of sentences formed.

Tests No. 14 and 15 suffer under the handicap of small range of scores. The steps between the scores are too large to reveal slight differences in achievement. In the cases of the sentences, the papers which contained a relatively large number of sentences

[17] Whipple, *op. cit.,* p. 441 ff.
[18] Whipple, *op. cit.,* p. 436 ff.

necessarily showed much sameness in subject-matter and structure.

Both these tests call for a certain amount of ingenuity and alertness. They are probably influenced somewhat by the size of the vocabulary at the command of the subject. The amount of work done in each of these five-minute periods is small in comparison with the length of time devoted to the tests. It would be desirable to arrange tests that allow greater amount of work to be performed in comparison with the time devoted to it. The results of the two tests do not correlate very highly with each other, as is shown by Table III p. 50. They hold similar positions however, in correlation with the scores of the tests combined.

Test No. 16. Business Ingenuity *g*

Materials: Mimeographed copies of the following "problem."

Mr. A. is in the manufacturing business—manufacturing knit underwear. He inherited the business from his father who was sole owner and proprietor. On the death of the latter, which occurred a year ago, the business, which aside from the homestead comprised the entire estate, passed into the hands of Mr. A., Jr., and his sister.

The elder Mr. A. had conducted the business for forty years with remarkable success. Under his wise and careful management it had grown from a small work-shop employing a dozen persons, to a plant of considerable proportions. Its employees now number 250; there are 10 salesmen, and the product of the mills is surpassed in quality by none on the market.

Although the plant had expanded greatly under the guidance of its founder, still for the past 10 or 15 years it has just been holding its own. Rival firms have been making great inroads upon its trade. The old gentleman scorned the artifices of modern advertising and otherwise refused to make any concessions to the cheaper trade, depending upon the continued excellence of "Excelsior" brand goods to win. Consequently, at his demise, the business was paying only moderate dividends.

With the removal of the powerful personality that had always dominated the affairs of the "Excelsior Knitting Mills," business fell off alarmingly. Salesmen daily reported the loss of old customers. The best salesman of the force tendered his resignation, having accepted a position with a rival house. Furthermore, Congress recently raised the tariff on raw wool, thus increasing the cost of production. In addition to these reverses, Mr. A. has been experiencing considerable financial pressure for several months. It is now the middle of January, and he is facing a crisis. Last summer, being hard pressed by his importing house for settlement of an overdue bill for raw material, he had gone to the bank and borrowed $8,000, giving two notes

for $4,000 each, one due in six months, and the other, in one year. The first note falls due February 1, two weeks off. He had expected to meet this obligation in February with remittances from his customers, who by this time should have turned over a great part of their winter stock. In this he was disappointed, however, as collections are extremely slow of late, being barely sufficient to cover the pay-roll. And now as Mr. A. sits at his desk, pondering over the difficulties that confront him, the aged bookkeeper who had served the firm for 25 years enters and sadly lays before him a statement from the bank, showing an overdraft of $900 for the last payroll. This is especially ominous, as another payroll is due in two weeks. Mr. A. greatly dislikes to shut down the factory. It constitutes the chief means of support for the town of 2,500 inhabitants. A severe winter is at hand, and it would work great hardship upon many families to throw his force out of work at this time. Besides, to close the factory would be disadvantageous to the business itself in more ways than one.

As Mr. A. studies the situation in all its phases, he sees that it is not perfectly hopeless. He has buildings and grounds worth $75,000, machinery and equipment worth $30,000—all in good condition. His books show bills receivable, amounting to about $10,000, but it should be said in explanation of this that he fears to collect any part of it by pressure, inasmuch as it has been his father's policy to be very lenient with his customers, and since patronage has already fallen off so markedly within the past year, he dares not risk any more defection by drastic collection proceedings. Perhaps his most valuable asset is the "Excelsior" trade-mark and the untarnished reputation of the house. This and other features of the situation give him encouragement, and he seeks the best course to follow.

There are several ways in which this situation might be met. Describe *briefly* all the solutions you can think of, any one or all of which might be used not only (1) to meet the present crisis, but also (2) to put the business on a good running basis.

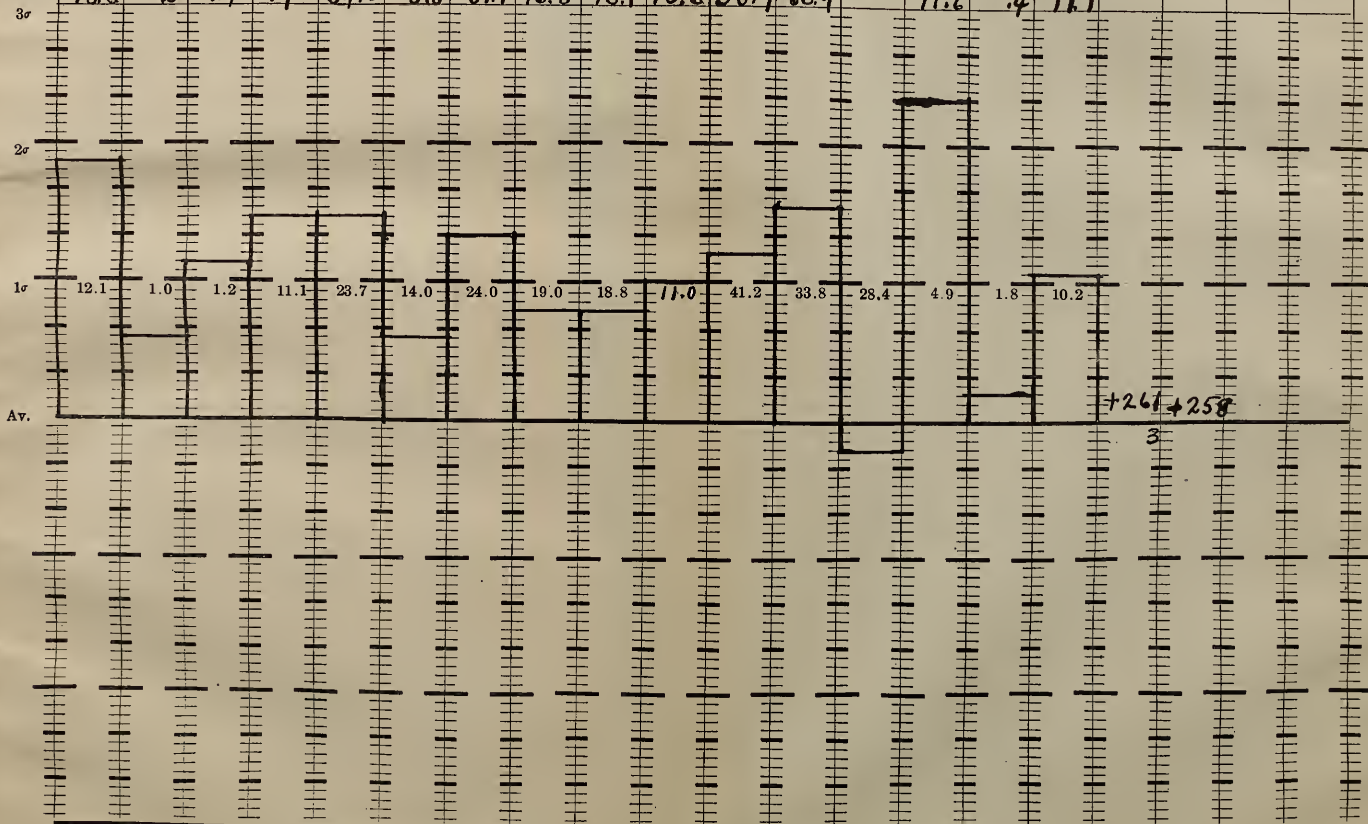
Directions: "Study the contents of this paper carefully and obey the directions given in the last paragraph. There is no time limit."

Method of Scoring: Caution was observed in scoring the results of this test, to eliminate any bias in evaluating answers. It would plainly be unfair for the experimenter to set a value arbitrarily upon each possible solution, as such evaluation would be based upon wider experience than that possessed by the members of the group tested. Neither would a mass judgment made by financial experts furnish an adequate standard. The fairest way seemed to be to count all the solutions offered and to grade each paper with respect to the judgment of the total group. This was accomplished by tabulating every solution offered and counting the number of times it was offered. The number of different

solutions offered was 46. Each of these was mentioned from 1 to 56 times by the 68 persons who took the test the first year. For example, 58 persons suggested "advertising campaign" along modern lines"; 45 suggested "mortgage some property"; one person suggested that the factory be closed temporarily, etc. The 46 solutions were offered altogether 363 times. In order to grade on a basis of 100, 100 was divided by 363, leaving .275 as a unit. It was then easy to evaluate the different solutions by multiplying the number of times each was mentioned, by .275. Thus the reply "advertising campaign" received a value of 15.9; "mortgage property," 12.4; "close factory, .275, etc. Each paper was then scored by crediting each solution with the value which the combined judgments of the entire group placed upon it.

NAME

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Test	Numbers Checked	Numbers Heard	Objects Seen	"Ideas" Reproduced Immediately from Hearing	Ditto after 2 Weeks	Ditto from Sight Immediately	Ditto after 2 Weeks	Loss in 5	Loss in 6	Opposites	Constant Increment	Instructions Printed	Instructions Oral	Words Built	Sentences Built	Business Ingenuity				
Highest	100	10	10	88	91	93	100	0	0	34 sec.	73 sec.	37 sec.	19 sec.	33	12	60				
Lowest	46	7	5	30	0	31	0	73	85	74 "	290 "	184 "	194 "	11	3	14				
Average	69.2	8.4	7.6	71.0	56.2	77.2	63.9	15.8	16.1	52.6 "	139.7 "	110.9 "	58.2 "	21.4	6.6	35.9				
Score	88	9	9	88	91	86	95	0	0	42	89	60	64	33	7	47				
Dev. −													5.8							
Dev. +	18.8	.6	1.4	17	34.8	8.8	31.1	15.8	16.1	10.6	50.7	50.9		11.6	.4	11.1				



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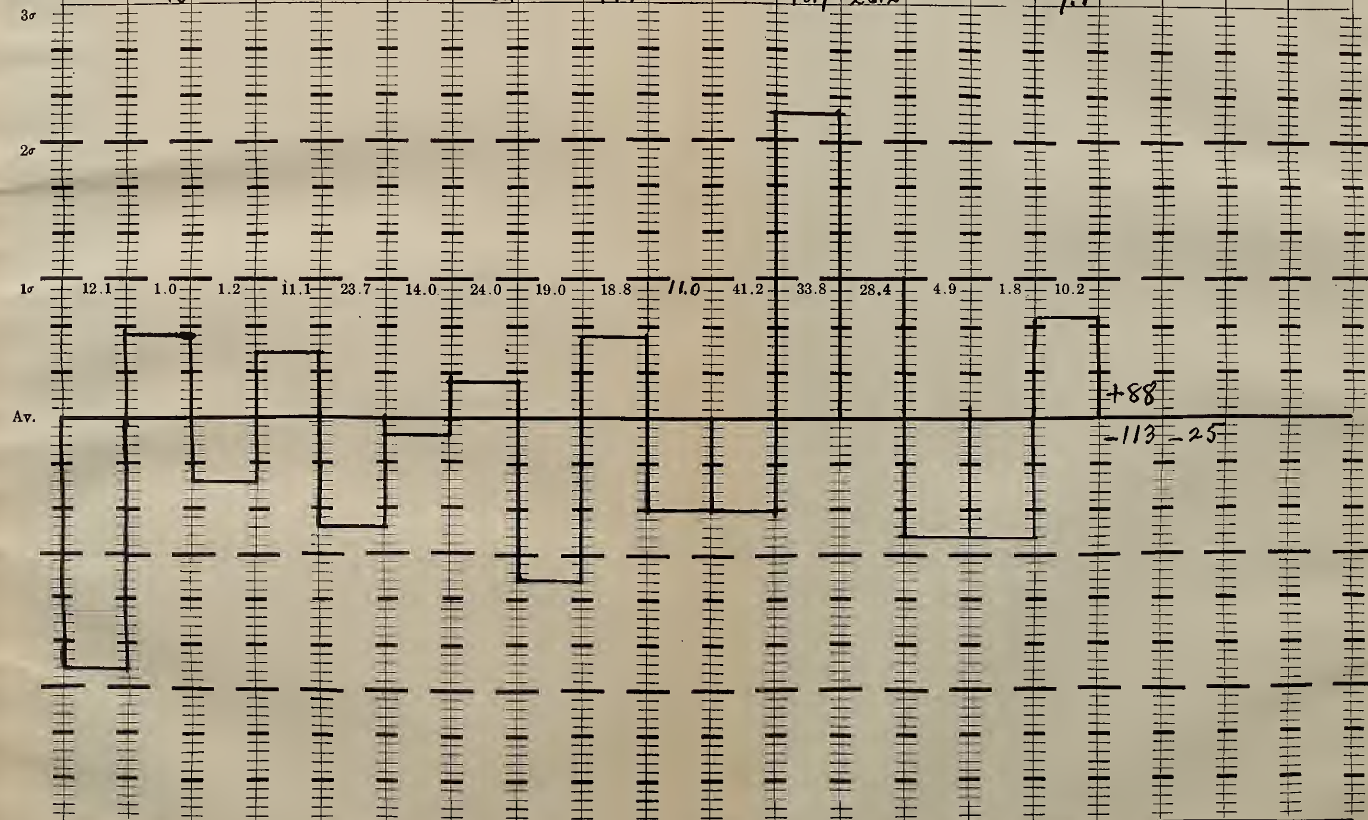
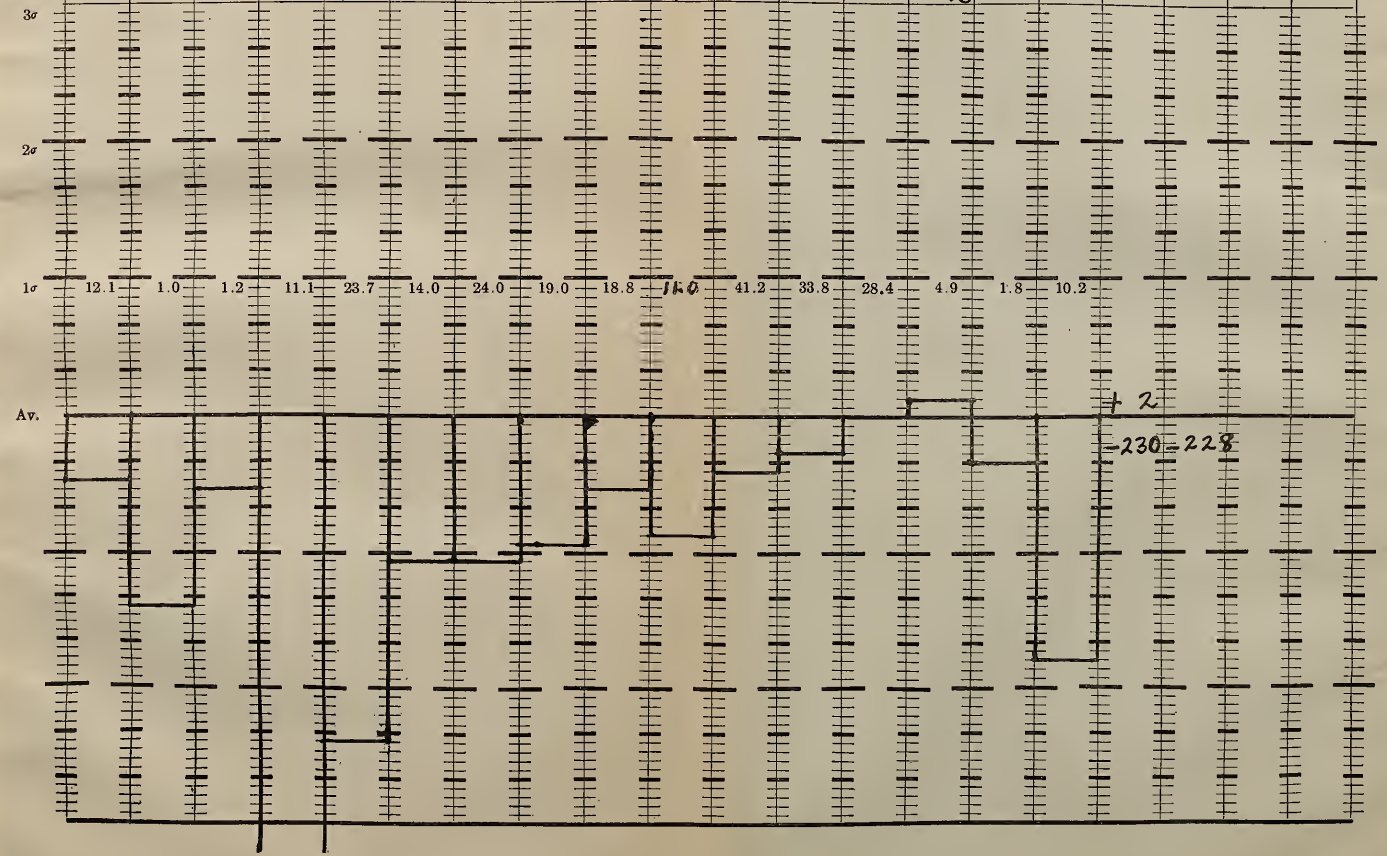
Name

Test	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
	Numbers Checked	Numbers Heard	Objects Seen	"Ideas" Reproduced Immediately from Hearing	Ditto after 2 Weeks	Ditto from Sight Immediately	Ditto after 2 Weeks	Loss in 5	Loss in 6	Opposites	Constant Increment	Instructions Printed	Instructions Oral	Words Built	Sentences Built	Business Ingenuity				
Highest	100	10	10	88	91	93	100	0	0	34 sec.	73 sec.	37 sec.	19 sec.	33	12	60				
Lowest	46	7	5	30	0	31	0	73	85	74 "	290 "	184 "	194 "	11	3	14				
Average	69.2	8.4	7.6	71.0	56.2	77.2	63.9	15.8	16.1	52.6 "	139.7 "	110.9 "	58.2 "	21.4	6.6	35.9				
Score	47	9	7	76	37	75	70	39	5	60	172	37	30	17	5	43				
Dev. −	22.2		.6		19.2	2.2		23.2		7.4	32.3			4.4	1.6					
Dev. +		.6		5.			6.1		11.1			73.9	28.2			7.1				



Name

Test	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
	Numbers Checked	Numbers Heard	Objects Seen	"Ideas" Reproduced Immediately from Hearing	Ditto after 2 Weeks	Ditto from Sight Immediately	Ditto after 2 Weeks	Loss in 5	Loss in 6	Opposites	Constant Increment	Instructions Printed	Instructions Oral	Words Built	Sentences Built	Business Ingenuity				
Highest	100	10	10	88	91	93	100	0	0	34 sec	73 sec.	37 sec.	19 sec.	33	12	60				
Lowest	46	7	5	30	0	31	0	73	85	74 "	290 "	184 "	194 "	11	3	14				
Average	69.2	8.4	7.6	71.0	56.2	77.2	63.9	15.8	16.1	52.6 "	139.7 "	110.9 "	58.2 "	21.4	6.6	35.9				
Score	62	7	7	34	0	62	37	34	25	62	155	119	59	22	6	19				
Dev. −	7.2	1.4	.6	37	56.2	15.2	26.9	18.2	8.9	9.4	15.3	8.1	.8		.6	16.9				
Dev. +														.6						



CHAPTER IV

Psychological Norms for College Students

Before employing this series of tests for purposes of practical diagnosis it was obviously necessary to establish norms of performance. The figures for such norms were obtained from the records of the first group to whom the tests were given. As described in Chapter II, forty students were found who had complete records, and the averages and medians for this group are shown in Table I.

TABLE I

Average and Median Score made in each test by 40 subjects.[19]

		Average	m.v.	Median
No.	1	69.2	10.3	71.5
"	2	8.4	.8	8
"	3	7.6	.9	7
"	4	71.0	8.2	74
"	5	56.2	18.6	61
"	6	77.2	10.5	79.5
"	7	63.9	20.1	63.5
"	8	15.8	15.0	9
"	9	16.1	15.4	8.5
"	10	52.6	8.8	53.0
"	11	139.7	32.0	135.0
"	12	110.9	26.7	105.5
"	13	58.2	17.6	54.5
"	14	21.4	4.1	22
"	15	6.6	1.4	7
"	16	35.9	8.6	37.5

In view of the practical use to which the records were to be put, it was desired to present each student's record in graphic form Accordingly a chart was arranged (see opposite page), which provided for graphic representation and at the same time gave means of combining the scores in the several tests so as to furnish a figure representative of the student's standing in all combined. Accompanying each graphic record were remarks of an interpretative nature, for in addition to the numerical results

[19] For further information regarding the distribution of these measures, see chart for individual records showing highest and lowest scores in each test.

obtained by psychological examination, it is also possible to secure much knowledge concerning the mental characteristics of an individual which are not expressible in numerical terms, but nevertheless are of considerable value in understanding his case.

As already stated, the graphic chart was arranged with two ends in view—first, to permit a clear and easily read presentation of the student's standing in each test, in other words, to show the distribution of his mental traits; second, to furnish a net score combining his standing in all the tests according to the third requirement set by Stern. The chart was constructed on the following plan: Each test, numbered from 1 to 16, is given a vertical column. Below the name of each test appears the highest score made, the lowest score, and the average. Below this, is inserted the score of the individual, and below this, the amount of his deviation from the average, either plus or minus, according as the deviation is meritorious or the opposite. Two-thirds of the distance down the vertical column is a figure which represents the Standard Deviation for the measures. Approximately in the middle of the chart is a heavy horizontal line. This represents the average and is the base line from which all deviations are measured. Each vertical column is divided into three large divisions on either side of the average line, and each of these divisions has three subdivisions which are again divided into five steps each. The gross divisions represent one, two and three times the Standard Deviation. An illustration will make clear the use of the Chart. Suppose a subject scores 89 in test No. 1. This means a deviation of 20 above the average (69.2). Twenty is once the Standard Deviation (12.1) and two-thirds as much again. Hence one large division is marked off and two-thirds of another. Counting in terms of the smallest subdivisions, the standing of the individual in the test is twenty-five units of deviation above the average. These units are equal throughout the series of tests, for the Standard Deviation[20] bears the same

[20] The Standard Deviation σ was used in this case according to the formula $\sigma = \frac{\sqrt{\Sigma(d)^2}}{n-1}$ where d= deviation from the average, Σ= the sum and n= the number of cases (40). Another measure of variability could probably be used as well. The chief aim is to have a relation that is constant between average and individual measure in all tests.

relation theoretically to the average score in each test. The net score for each individual is obtained by adding the units of deviation above the average and those below the average, and subtracting the smaller from the larger sum. The number thus secured is the plus or minus score of the individual and gives numerical basis for designating his rank in the group. It should be kept in mind that these quantitative expressions of performances in the tests serve in no way to express the mentality of the individual in absolute terms. The net score is merely a resultant value of all the tests in the series, and meets the third requirement set by Stern. In its essential nature, however, it is only a relative measure and places the individual only with respect to the other members of his group. The ranking of the subjects on the basis of net score is shown in Table II.

TABLE II

Net Scores representing standing in 16 tests pooled for 40 individuals.

+259	— 14
+135	— 18
+119	— 29
+106	— 30
+ 81	— 31
+ 75	— 39
+ 73	— 45
+ 67	— 53
+ 65	— 61
+ 65	— 66
+ 64	— 77
+ 63	— 82
+ 44	—102
+ 41	—107
+ 41	—116
+ 32	—131
+ 31	—160
+ 20	—237
+ 18	—255
+ 17	
+ 3	

These net scores when arranged from highest to lowest follow the normal curve of distribution, twenty-one being above and nineteen, below the average. The extremes are also approximately at the same distance from the average. In examining the individual charts, it is found that they range from conditions where the deviations in all the tests are almost entirely above the average to conditions where the deviations are practically all

below the average. By far the greater number of individual charts, however, show the deviations to be partly above and partly below the average. This is as might be expected in view of what is known about the distribution of mental traits in most individuals. Inasmuch as it is impossible to present all the individual charts, three samples are shown—representing the three types of distribution (p. 45).

It will be noted that the construction of the chart assumes that the tests are equal in their demands upon intelligence on the average.[21] This does not mean, however, that memory for digits, for example, is equal to speed of giving logical associations, but that the amount of deviation proportional to the Standard Deviation in one test is equal to the same amount of deviation proportional to the Standard Deviation in another. This assumption seems justified in view of the relativity of the measures, and the fact that the unit of measure is based upon the Standard Deviation for all tests, which presumably holds a constant relation to the several averages. Another assumption on which the chart is based is that excellence is always to be found on the plus side of the base line. It assumes, for instance, that to have a poor memory for objects visually sensed is a psychical dereliction. This may appear to be unwarranted, inasmuch as a person may have poor ability to reproduce what he has visually apprehended, and still display marked intelligence. He may, for example, have habituated himself to some other form of sense impression. A similar assumption penalizes slowness of response, making it appear that the individual who is slower than the average has less intelligence, whereas some psychological doctrine regards speed as purely a matter of individual variation. These practical questions involve weighty problems regarding the theory of intelligence and this is not the place to discuss them. Still it is pertinent to suggest that in the long run, for meeting practical situa-

[21] For discussions of similar methods of amalgamating scores see R. S. Woodworth. Statistical Method, Psychol. Rev. 19, 1912, pp. 97-123; A. P. Weiss, A. Modified Slide-rule and the Index Method in Individual Measurements, J. of Educ. Psychol. 5, Nov. 1914; A. R. Abelson, The Measurement of Mental Ability of Backward Children. Brit. J. of P., 1911, 4, 268-314; W. Stern, Differentielle Psychologie, Barth, Leipsig, 1911. p. 17 ff.

tions in life, the most serviceable type of mind is that which shows excellence in each of these divers powers. Furthermore, according to the trend of thought and practice regarding mental tests, such varieties of excellence are provided for by the use of a large number of tests, and the increase in the number of different traits tested minimizes the danger of specialization. This introduces what Stern characterizes as a "systematic compensation"[22] mechanism which provides for qualitative differences in the same degree of intelligence. Some justification for this view is furnished by the fact that the measures in Table II conform to the normal curve of distribution; and further justification for the method will appear when the results of the pooled tests are compared with judgments of mental ability.

Inter-test Correlations

In attempting to show the relations among the results of a series of tests the method of correlation has been much employed. The results of different tests are compared and the degree of correspondence is stated in terms of a "correlation coefficient" which is the "measure of the tendency towards concomitant variation exhibited by two series of phenomena and hence throws some light upon the causal relations of these phenomena."[23] Adopting this method, a number of correlations were computed between results of tests that might be expected to be closely related. The correlation coefficients are shown in Table III. Considering a correlation high only when its coefficient is four or five times as large as the P. E., a significant positive correlation appears only between (1) memory for meaningful material seen and heard, (2) between the first and second reproductions of this material, and (3) between the Opposites and Constant Increment tests. Further inter-test correlations were abandoned because at this stage of the work it seemed more profitable to direct the major attention to other phases of the problem. Other recent investigators have evinced considerable dissatisfaction with dependence upon inter-test correlations in judging the validity of a series of

[22] *Op. cit.*, pp. 21, 22.

[23] Wm. Brown, The Essentials of Mental Measurements, Cambridge, University Press, Cambridge, 1911. p. 47.

tests.[24] It is clear that a high degree of correlation should not be expected if the series is a good series calling for varied mental processes. In the present series some of the tests stress memory of various kinds, others, reasoning processes, some call chiefly for display of originality and initiative in various fields, others emphasize sense differences. They vary also in amount of motor activity involved. For these reasons if a high degree of correlation were found among all the tests, their value might be seriously questioned; one would be inclined to doubt that they reached different phases of mentality. Therefore, in a series such as this, a certain variety is a virtue, first because it helps to give a more comprehensive view of the qualitative distribution of mental traits in any individual; second, because when a person holds a decidedly high or low position in several tests, the reliability of the measures increases with every deviation that follows the general trend.

TABLE III

Inter-test Correlations Camputed by the Product- Moment Method[25] (Raw)

	r.	P.E.
Number-checking and Hard Directions (printed)	.01	.10
Numbers Heard and Objects Seen	.09	.10
Numbers Heard and Directions (oral) (accuracy)	—.05	.10
Numbers Heard and Logical Material Heard	—.09	.10
Objects Seen and Logical Material Seen	—.13	.10
Logical Material Heard and ditto seen (immediately)	.26	.10
Logical Material Heard and ditto seen (deferred)	.54	.07
Loss in Logical Material Heard and ditto Seen	.54	.07
Logical Material Heard (first and second reproductions)	.49	0.8
Logical Material Seen (first and second reproductions)	.45	.08
Opposites Easy and Hard (Speed)	.46	.08
Opposites Easy and Hard (accuracy)	.51	.07
Opposites Easy (speed) and combined Index	.84	.03
Opposites Easy (index) and combined Index	.84	.03

[24] *Cf.* Abelson, *op. cit.* In a series of tests similar to these this author found the inter-test correlations of little interpretative value, and was obliged to use other means of evaluation, employing Spearman's valuable method of pooling. This author concluded that the differences among the tests are negligible anyway, and that they are "about equally accurate measurements of general ability." p. 302.

Also *cf.* E. Webb, Character and Intelligence, Brit. J. of P., Mon. Sup. Vol. I, No. 3, p. 83.

[25] According to the formula $r = \frac{\Sigma(xy)}{n\sigma_1 \sigma_2}$

Opposites Hard (index) and combined Index	.86	.03
Opposites Easy (speed) and Constant Increment (speed)	.40	.08
Opposites Easy (accuracy) and Constant Increment (accuracy)	.10	.10
Opposites Combined Index and Constant Increment Index	.38	.09
Hard Directions Printed (Speed with accuracy)	.36	.09
Hard Directions Printed (Speed with index)	.84	.03
Hard Directions Printed and oral (speed)	.23	.10
Hard Directions Printed and oral (accuracy)	—.02	.10
Hard Directions Printed Index and Oral Index	.23	.10
Hard Directions Oral (speed and Index)	.95	.01
Hard Directions Printed (speed and accuracy)	—.11	.10
Word-building and Sentence-building	.05	.10
Word-building and Business Ingenuity	.10	.10
Sentence-building and Business Ingenuity	—.02	.10

Abandoning the search for the significance of inter-test relationships, further observation of the measures reveals a peculiar phenomenon of mental life. It has been found by several investigators that measures secured from several tests, though singly not correlating highly with other series of measurements, nevertheless, when amalgamated may correlate highly with other series. This phenomenon has been noted and discussed by Abelson,[26] who, by a process of pooling, showed that averages from a number of tests which taken singly, showed little correlation with other measures of ability, nevertheless, when pooled, correlated with a high magnitude. As explained earlier in the chapter the method of graphic representation furnished a way to combine the scores made by an individual in the several tests, and to express the standing in all the tests in a single net-score. The net-scores made by the forty persons in the experimental group were correlated with their scores in each of the tests and it was found that much higher correlations obtained than between the scores of the separate tests. The results appear in Table IV.

TABLE IV

Correlation of Standings in Each Test with Standings in Net Score. (Method of Rank Differences[27])

			ρ	P.E.[28]
Test No.	7	Logical Material Seen Deferred	.60	.07
" "	10	Opposites	.53	.08
" "	12	Hard Directions (printed)	.49	.08

[26] *Op. cit.*

[27] According to the formula, $\rho = 1 - \frac{6\Sigma(d)^2}{n(nxn^2-1}$

[28] According to the formula P.E. $= \frac{.7063(1-\rho^2)}{\sqrt{n}}$

"	"	3	Objects Seen	.48	.08
"	"	9	Loss in Logical Material Seen	.47	.09
"	"	5	Logical Material Heard (deferred)	.45	.09
"	"	14	Word-building	.45	.09
"	"	8	Loss in Logical Material Heard	.43	.09
"	"	15	Sentence-building	.42	.09
"	"	11	Constant Increment	.38	.10
"	"	16	Business Ingenuity	.33	.10
"	"	6	Logical Material (immediate)	.29	.10
"	"	2	Numbers Heard	.27	.10
"	"	13	Hard Directions Oral	.23	.11
"	"	4	Logical Material Heard (immediate)	.23	.11
"	"	1	Number-checking	.18	.11

In view of the uncertainty attending the meaning of inter-test correlations, it seemed wise, as Stern[29] suggests, "to seek the means of gauging the tests in some criterion that lies outside of the experiment." As has already been shown, the conditions surrounding this investigation furnished unusual opportunity for linking up the results with the other relations of the student. Of course much of the data gathered by the college office could hardly be reduced to mathematical terms. For example, it would be difficult so to treat judgments of employers and teachers and the information regarding social, and physical activities. These were for the most part in descriptive terms. One group of extra-laboratory measures readily available, however, consists of university grades and they will next be considered.

Correlations between College Grades and Results of Tests

The index of correlation between standings in university grades for the year 1913-14 and standings in the psychological examination was found to be .44 (P. E. .09) using the Pearson method adapted to rank differences. Though this is a positive correlation it can not be considered high. In terms of percentage, of the 20 individuals in the better half of the ranking according to grades, 13 or 65% are also in the better half of the ranking according to scores in the tests. The use of the same tests with the 1914 Freshmen gives opportunity for further correlation with university grades. This group furnishes 40 complete records. The scores made in the tests correlate with grades for the year

[29] *Op. cit.*, p. 115.

1914-15 by .20 (P. E. .11) using the Pearson method adapted to rank differences. Of the 20 individuals in the better half of the ranking according to grades, 11 or 55% are in the better half of the ranking according to the tests.

These results give point to the remark that the correlation between intelligence as measured by tests and academic standing is not so high as is popularly supposed. An analysis of the conditions of university life shows that many other factors besides intelligence enter in to determine class-room standing. One prominent source of error now under experimental scrutiny is the subjectivity of instructors' gradings. Another large group of factors may be traced to the student himself. Some of these have to do with will in the widest sense of the term. If a student shows decided ability in psychological tests and his university work is poor it may be found that his faults are in the direction of moral qualities. Or if a student achieves only moderate success in the tests but does distinctly good work in the university, then "there is a probability . . . that this pupil's strength is to be sought primarily in qualities of character and will."[30]

Still other conditions affecting the student's grades are social surroundings. Especially is this true when students live at home, and in the case of a city institution, this becomes a serious problem. Likewise economic conditions affect the student's grades. If he must earn his way through the university, it is manifest that his academic standing will be affected by the fact. On the other hand, if he has too much money at his disposal there is danger that his university work will suffer. A final consideration is the physical condition of the student. His bodily well-being colors largely his university work.

When one considers the complex conditions of the marking system in college or university, one is not surprised to find low positive correlation between the results of psychological examinations and college grades. This lack of correspondence blights some of the hopes cherished for psychological tests—the hopes that they may serve as entrance examinations, to foretell what kind of college work a student will do. Psycho-

[30] Stern, *op. cit.*, p. 64.

logical tests are meeting with such favor, and are proving so useful as indicators of mental ability that one can readily understand the desire to use them to sift out the undesirable applicants at colleges and universities. Such an arrangement seems especially desirable when one considers the large number of students who are dismissed at the end of their first term in college. In view of the deplorable waste involved in thus accepting students only to be dismissed as incompetent, it would be a great boon to humanity if psychological examinations could thus be employed. The gain would be both to the university and to the individual. Such sanguine expectations, however, will probably not be realized, for even with a perfectly flawless series of psychological tests, there is one group of factors that can not at present be measured—those things designated moral and volitional qualities.[31] A student might prove superlatively bright in his entrance examination, and still fail in academic work because he was not strong enough to withstand the allurements of extra-curriculum attractions. Furthermore, prophecies concerning the future would also be out of place because of the multitude of other factors discussed mentioned above. In truth, however, it must be pointed out that the present type of entrance examinations, based as it is, on strictly academic grounds, is not any more infallible in sifting out the undesirables as is evidenced by the number of eliminations that occur among Freshmen. It is not at all impossible that a system might be evolved that would employ both academic and psychological tests, and while the combination would not be absolutely perfect in designating fitness for college or university work, it should nevertheless be better than either kind alone.

Correlation with Estimated Intelligence.

In order to ascertain the value of the tests by another means, it was decided to rank the students according to their estimated intelligence. The method employed was as follows: The students in the first experimental group of forty were ranked ac-

[31] The recent work of Webb (*op. cit.*) suggests that future developments may disclose means of measuring some of these heretofore elusive characteristics.

cording to their University grades, then the dean was asked to correct this ranking on the basis of all the information at his disposal, so that the arrangement should represent standing in intelligence as purely as it was possible to abstract it. The question asked was, if all these students had an equal chance in other respects, how would, they rank in practical intelligence—intelligence being regarded as ability to adapt one's self to new situations?[32]

These two series of rankings correlate by .57 (P. E. .05) using the Pearson method adapted to rank differences. In terms of percentage, of the 20 students in the better half of the ranking according to estimated intelligence, 13 or 65% are in the corresponding half of the ranking according to the tests. It would have been desirable to make a similar comparison between these two measures of the 1914 Freshmen but the dean felt that one year of acquaintance was too short to permit satisfactory estimation of intelligence. Even with the first group of students, though the acquaintance at time of making the estimates was in no case less than one year and in some cases extended over two and three years, still considerable perplexity was experienced in deciding upon just rankings. One of the factors inimical to success in this work is the liability to bias by grades. Thus it happened that office contact is closest with those students who receive low grades. Quite without reference to the other facts of the situation one is prone to assume that such students are low in intelligence. Other factors entering into estimations of intelligence are differences in length of acquaintanceship, reticence on the part of individuals and the briefness of contact with the students. In spite of these well-recognized artefacts, however, the correlation is quite decidedly significant. It agrees closely with that found by Abelson[33] who, after pooling the results of nine tests and correlating them with estimates of intelligence, found a coefficient of .60 for girls and .56 for boys.

The main purpose of this report is not to advocate the adoption of this particular series of tests for use in educational guidance.

[32] Stern, *op. cit.*, p. 3.

[33] *Op. cit.*, p. 303.

Its prime concern is with the establishment of a method. Therefore little can be gained by statistical studies of the tests alone or in correlation with one or two external factors. The hypothesis is that the entire individual must be taken into consideration and that psychological measurements are an integral part of this all-round examination—not as absolute or self-substantiating values, but as measures coordinate in interpretative value with measures of the student from other aspects. In support of such a contention the best proof that can be adduced will consist in the demonstration of the usefulness of the tests in actual administrative situations. What service do they render in the concrete situations that arise in administering the education of individuals under conditions such as those described? The report of this practical trial of the tests will be given in Chapter VI. First, however, should be pointed out the possible uses of such tests with various college groups, and this will be assayed in Chapter V.

CHAPTER V

Comparison between College Groups

One of the most desirable benefits of a standard system of psychological examinations for college students is the opportunity for making comparisons between various groups of students. It is very desirable to measure the psychological differences between various entering classes, between various graduating classes, to measure the differences between the performance of a group during the Freshman year and performances at later stages of advancement, between students of various colleges, departments, etc. The effects of certain branches of the curriculum might also be studied, as well as the results of the teaching of various instructors.

The data at hand throw some light upon one question of immediate interest—namely, the determination by objective means, of the effect of university training upon the mental capacity of a group of students. In order to investigate this problem the members of the 1913 group who were still in the university and who varied around the Sophomore point by no more than three months, were retested in 1914-15. The group consisted of 21 persons; 16 were men and 5, women. The average age on October 1, 1914 was 20.0 years. On March 1, approximately a year from their former testing as Freshmen, these students were called together and examined with tests Nos. 1, 2, 14, 15 and 16. Tests Nos. 10 and 11 were given individually during the week March 1-6. It was impossible to use the other nine tests of the series for this second testing since there was danger that memories held over from the previous year might be of service. The scores made by the members of this group at two stages of development are shown in Table V, together with the average scores made by the same individuals as Freshmen.

TABLE V

Average Scores in 7 Tests Made by 21 Freshmen (1913)

Test No.	1	2	10	11	14	15	16
Av. score	72.2	8.2	52.8	141.5	21.6	6.6	37.2
M. V.	8.8	.8	4.0	34.8	4.9	1.4	8.5
P. E. of Av.	1.6	.2	.7	7.4	.9	.3	1.5

Average Scores Made by Above Group as Sophomores (1914)

Test No.	1	2	10	11	14	15	16
Av. Score	75.8	8.2	46.0	124.9	21.9	9.6	40.3
M. V.	13.5	1.0	7.8	32.4	4.0	1.4	7.0
P. E. of Av.	2.0	.2	1.4	6.0	.7	.3	1.2
Absolute Gain	3.6	0.0	6.8	16.6	.3	3.0	3.1
Percent Gain	5.0	0.0	12.9	11.7	.1	4.5	8.0
P. E. of Diff.	2.5		1.5	9.5	1.1	1.3	1.9

In the results of the second year's testing, there is no score below the corresponding score for the previous year. Improvement is shown in every test except in Test No. 2 (Numbers Heard). As a measure of the reliability of the difference between the two series of averages, it is necessary to refer to the probable errors of the differences. These are found to be less than the difference in every case except that of Test No. 14, showing that most of the differences can not be accounted for by mere chance. The certainty of a true difference is further evidenced by the fact that the differences between the two series of scores are in the same direction for all the tests.

The data do not show whether the improvement is due to influence of maturity or to general training. This question awaits investigation by use of control groups.

It is hardly possible from this limited amount of data to make a statement regarding the relative improvement in different mental traits. Attention is called, however, to the unchanged score in Numbers Heard a rote memory activity, which seemed less susceptible to influence by the factors operating during the year, whether they be related to maturing of ability or to university training.

The above figures represent improvement distributed among the various tests. Another view of this improvement may be secured by considering the net score made by each individual when his scores in the seven tests are pooled. Comparison be-

tween these two series of net scores for the two years shows a correlation of .88 P. E. .03. This high correlation, indicating that the individuals tended to keep the same relative ranking during the year, may signify two things. If there were no disturbing factors, this correlation coefficient might represent a reliability coefficient signifying that the tests give reliable measures of ability, inasmuch as a second measurement makes little change in the relative standings. There is a disturbing factor, however,—lapse of time, wherein the subjects were under university training among other environmental influences. This training had been quite similar both in quality and quantity. In view of this homogeneity of the group, then, the high correlation coefficient may indicate that the members of the group improved with relatively equal amounts.

It is to be regretted that the entire series of sixteen tests could not be used for the second testing, but it was felt that all but these seven would be colored by memories from the previous year. This predicament shows that in applying tests to this problem, care should be taken to choose tests that can be repeated without danger of memories being held over that might be of assistance. In cases where this is not possible, as in logical material to be reproduced, it will be necessary to arrange tests that are approximately equal in difficulty, interest-value, etc. Such tests await development in psychological laboratories.

Another feature to be developed is the preparation of charts for the presentation of individual records. It is necessasry to arrange these charts so as to show not only the relation of the scores to the average of the group, but also their relation to records of previous years. In the present study only one kind of record-blank was used—all norms with which to compare the individual records for graphic presentation being secured from the records of the first experimental group—predominantly Freshmen.

Considerable interest attaches to a comparison of the two Freshman groups that have been studied, and an attempt was made to compare their records. It was thought, however, that such a comparison would be valueless because the tests were

made during the middle of the school year (February) in the case of the first group, while they were made near the opening of the school year in the case of the latter group. This resulted in making the two groups different in that the first group had been subjected to a selective process and the poorer students, or at least, those with very low grades, had been eliminated to some extent at the end of the first quarter's residence. Furthermore, on the supposition that a short period of university training may produce improvement in general mental ability, the two groups would be otherwise unequal, the first group having received four months of university training of which the second group had not had the advantage.

It should be emphasized that such manipulation of date as that just described is not done for the purpose of securing answers to the questions posited at the opening of this chapter. The measures here presented are too few to permit such conclusions. The calculations have been made merely to exhibit the uses of the method and to show how such measures may ultimately serve.

CHAPTER VI

Training for Efficiency in College

The series of tests here described is not being urged as a flawless adjunct to university administration. The hypothesis merely sets forth the possibility that psychological tests may be useful in facilitating the educational guidance of college students. The proof of this hypothesis is difficult of presentation in quantitative terms. In the first place the results of educational practice must necessarily await the test of time before they reach their highest fruition. Second, at present there is no accepted standard for measuring the most important results of education such as improvement in moral strength, power of appreciation, etc. It is clear, then, that the value of psychological tests in this field can not be expressed by a single figure, nor by a set of figures. The data found in the preceding chapters are offered in order to show that there is some basis for favorable judgment regarding the method, and to give some idea regarding the significance of the tests as a series. The chief justification for the use of the tests, however, must be of a pragmatic nature—must come from the help they have given in the guidance of college students through an individualized course of instruction.

Before proceeding to an exposition of this angle of the case, another feature of the plan must be described. This consisted in supplementing the psychological examinations by a series of lectures on methods of study. These were given to the 1914 Freshmen and aimed to present economical and effective methods of studying Freshman subjects.[34] Methods of memorizing were discussed and were applied specifically to the preparation of lessons in German, history, public speaking, etc. Other topics considered were habits of attention, and phases of the learning process. Also, by way of securing a clearer conception of the learning process some instruction was given in elementary neu-

[34] See the author's book, How To Use Your Mind (Lippincott).

rology. These lectures served in some degree to aid the students in orienting themselves during the first few weeks of trying adjustment to university environment, and especially to university methods of instruction. In addition to these general discussions adapted to the entire Freshman group, individual conferences were held wherein those students who found themselves in academic difficulties were given advice adapted to their specific problems. In this analysis of academic difficulties the habits of the student were carefully studied and the psychologist pointed out erroneous and uneconomical methods of study and gave specific directions for the formation of more efficient habits of mental application. As may be expected the kinds of advice required were of great variety ranging from suggestions embodying simple methods of memorizing, to the arrangement of an entire daily schedule.

In these individual conferences it may easily be seen that the psychological examination records are of great service. For example, if a student is found to have developed more effective methods of immediate reproduction than methods of deferred reproduction his reviews may be arranged accordingly. Furthermore, the tests seem to give some notion as to the amount of work a student is capable of carrying advantageously—all in terms of comparison with the average performance of his group. The tests may thus be helpful in avoiding the overloading of slower students and in spurring on the more able but lazy students.

The handling of students under this régime of personal supervision might easily degenerate into charlatanism especially in view of the preliminary stage of development which psychological consultations enjoy at the present time. In order to guard against this the impression was conveyed that the psychological examinations simply reflected mental condition as it existed at the time the examination was made; that the tests were not perfect measures of innate mental capacity; that they were of service mainly in revealing the methods of mental application which had been developed in the preceding environment. Assurances were given that none of the measures were absolute, but that all were relative to the group of students as a

whole; that relief from an unfavorable situation might be expected to take the form of development of more efficient habits of mental application—an aim within the reach of all, and finally, that the Freshman year was the most advantageous period for the formation of these habits. The results of these conferences can obviously not be measured, but the students seemed to find them helpful, and in view of the impressibility of Freshmen it is probable that these methods served to impress upon the students the importance of the first year of college life more emphatically than could be done if they were left to learn in a hap-hazard manner.

Conferences on study methods are very expensive in time, and require the exercise of considerable psychological insight. The light that is thrown upon Freshman difficulties, however, gives ample justification for their adoption. Some of the atrocious methods of study revealed in such conferences would open the eyes of administrators to the fact that an enormous waste occurs in the education of college students and that much precious energy is misapplied. The psychological fallacies disclosed emphasize the need of having a consulting psychologist at the disposal of every college student. The possibilities of training for study have never been demonstrated but it is safe to predict that the education of the future will adopt it as one of its salient features. In such event psychological tests will constitute a necessary part of the machinery. It is now apparent that each part of the system herein described is dependent upon every other part.

One gratifying result of the system as operated for the past two years was the effect upon the attitude of the students. They welcomed the attempt to measure their mental capacity, seeing in it an expression of the desire to be of service, which the College of Commerce and Administration has steadily manifested, and they responded readily. The disciplinary value of the tests became evident in dealing with individuals who were suspected of wilfully slighting university work. Examination of their environmental conditions revealed nothing that could explain their academic delinquencies. When the records in the

psychological examinations showed them to rank on the average or above in the mental traits tested, the administrative officer felt justified in charging them with lack of application, and in the face of the objective evidence of the tests the students admitted the charge to be just. After the exposure of one or two such cases, careless students tend to be less secure in their excuses for poor college work on the grounds of lack of ability. On the whole a very desirable frankness has been engendered between the students and the administrative office since the adoption of the methods herein described.

One unique function served by these records remains to be mentioned. Owing to the objectivity of this kind of measure, the rulings of the university may now be based on quite tangible evidence and in dealing with obstreperous parents, such an objective record as that from a psychological examination is very effective in supporting the position taken by the university, especially when it is accompanied by other measures, all of which point to the same decision.

The psychological records have been prepared for 143 students during the past two years and it is impossible to enumerate all the uses to which they have been put. Probably the greatest utility has centered around the delinquents. The question that most often perplexes the educator is "Why does this student fail"? To answer this question it is necessary to utilize all the facts that can be secured, and the results of psychological examinations, when technique is suitably developed, will constitute a vital part of this information. A few typical cases will be described showing how the tests are used in conjunction with other data of a social and economic nature.

Case A

This student came to the university with high recommendations and proceeded on the whole to justify them, making an exceptionally good record during his Freshman year. Of the forty students in the experimental group he ranked fourth in academic standing and third in the psychological tests. In his Sophomore year, however, his academic record was considerably

poorer. From all the evidence it appeared that he was working just as earnestly as during the preceding year. He had joined a fraternity but its influence was not derogatory so far as could be determined. A psychological reason for the inferior quality of his Sophomore work was suggested by an analysis of his psychological record and reports from his instructors. The analysis showed that so long as this student was studying subjects requiring the exercise of rote memory he encountered no difficulty for in this respect he possessed great ability. Freshman subjects favor this ability but in Sophomore subjects the student is encouraged to place less and less reliance upon it and to give more prominence to selective thinking. When this student was plunged into such conditions he found himself singularly inefficient. Instructors attributed his low grades to a lack of ability to carry out lines of thought involving selective thinking; the student remarked upon the same difficulty. All the evidence gave a reasonable hypothesis for the explanation of the discrepancy between the work of the two years. Special attention is being given to the development of the phase in which he is deficient.

Case B

This young lady came to the university with high recommendations and quite a fund of practical experience. All reports showed her to possess an exceptionally high grade of mental ability, and in the psychological tests she ranked first by a very high margin. In academic work, however, it was found that at the end of her Freshman year she ranked eleventh in the list of forty. Examination of this student from the all-round view afforded by the system of administration in operation showed that she was obliged to earn her own living and that she had been spending on the average five and six hours a day in outside work. During one quarter she spent forty-eight hours a week in outside work (though part of it was light), and carried regular university work (fifteen hours). In addition to this she devoted some time to student organizations. In this case the tests together with the other machinery of the system gave the basis for a more exact

estimate of ability than could be secured by observation of classroom work.

Case C

This young lady ranked last in the tests and thirtieth in academic standing in the group of forty. Her university instructors reported her lacking in mental power. At the end of the Freshman year she stood two points below the minimum standard set by the university and undertook the Sophomore work on a decidedly probationary basis. Her efforts were quite beyond reproach so far as earnestness of purpose and intensity of effort were concerned. Social and economic conditions were favorable to good work. The conclusion seemed forced that the difficulty was simply inaptitude for study. The story told by the psychological test was corroborated by all other evidence procurable and her withdrawal from the university seemed eminently justified.

Case D

This young man appeared from his recommendations to have distinctly more than average ability. In the psychological tests he ranked 14.5 among forty individuals. In academic record, however, he ranked thirty-fourth. Investigation along the lines of the office routine showed that he was concentrating only a small fraction of his attention upon academic things. He was popular among his fellows and participated excessively in extra-curriculum activities. His record was good enough to meet the minimum requirements of the university but it was decided to bring pressure upon him in accordance with the policy to demand from every one according to his ability. The hypothesis was that this individual had more ability than he was manifesting in university work. Accordingly the whole matter was laid before him in words somewhat as follows: "All the evidence that can be gathered concerning you points to the conclusion that you are capable of doing much better work than you are now doing. The question now at issue is the question of your return next year. Frankly it seems unwise for you to return next year. If

your environment here is such as to lead to the sort of record which you made last year it would seem wise to change the environment. If after thinking the matter over, however, you feel that you wish to return to the institution, it can only be on a probationary basis with the definite understanding that you will not be eligible for participation in outside activities and that your connection with the university may be severed at any time your work proves unsatisfactory. For an entire quarter nothing short of seven grade-points for three majors taken will be accepted as satisfactory."

The student decided to return in the autumn under the above conditions. Under the new régime he secured ten grade-points and in the winter quarter, eight more, with universal reports of improvement. The psychological tests were a powerful argument with this young man in convincing him of his own powers and of the desire of the university to draw out all the possibilities within him.

Case E

This student entered the university on representations from teachers and others that he was at least of average ability. In the psychological tests, however, he ranked thirty-ninth out of forty. Instructors in the university made discouraging reports as to his mental ability. In the effort to give him a better chance his work was cut down from fifteen hours to ten hours a week, still he made only a grade of C (75) in the two remaining courses. All the evidence showed that he was working hard, therefore he was permitted to continue in the university on a probationary basis. He remained throughout the year without improvement and inasmuch as the Sophomore work promised to be even more taxing on his powers he was advised not to return. Thus the hypothesis furnished by the psychological test record was confirmed by a year of university work.

Case F

This student entered the university with statements from high-school instructors which indicated that he was about an

average student with good fundamental moral qualities. The record he made in the psychological tests ranked him thirty-eighth in the list of forty students. In his first quarter of university work he secured but three grade-points and was placed on probation. The work of the next two quarters resulted in a total of only eleven grade-points so he was asked to withdraw. The parents came to the office in a rather belligerent frame of mind and asked on what ground the university took its stand. The information that had been gathered was laid before them and the parents were considerably enlightened when shown this many-sided view of their son. They departed with a better understanding of the youth and with confidence in the good intentions of the university.

The foregoing typical cases are representative of a great variety of situations in which the tests were clearly serviceable in adjusting students to their work and in determining the attitude of the university toward them. It is impossible in such brief statements to show the extent to which the psychological records were of service. Especially is this true when as in this report, the effort is made to express everything with extreme moderation. Only one accustomed to deal with the perplexities of university administration can fully appreciate the value of every bit of information about a student.

It is now time to summarize the results of this two years' trial of psychological tests as instruments for university administration. A series of sixteen tests has been employed, carefully chosen in the light of past experience in this field so as to give objective measures of mental ability. Care was taken to select tests that were well adapted on the symptomatic side as well as from the standpoint of economy and efficiency; an arrangement was provided for uniting the various results of the tests into a resultant value which would be a quantitative expression of the standing of an individual. The particular tests employed in this preliminary study lent themselves fairly satisfactorily to the needs of the situation. When gauged by the methods ordinarily applied to such tests they show a fair degree of conformity to

standards of efficiency. The distribution of the measures of the separate tests follow for the most part the normal curve of distribution, likewise the distribution of measures in the combined scores. The results of the tests correlate positively with college grades and to a closer degree, with estimated intelligence. By means of the tests the improvement in certain mental functions during one year of university training was measured. Lastly this psychological measure has been found useful in furnishing hypotheses regarding perplexing cases of students, which when acted upon provided happy solutions in many cases.

The thesis upheld here does not advocate the universal adoption of the particular tests in this series. Their imperfections are numerous and have been freely pointed out. The norms of performance here presented do not aspire to rank as standardized measures. With even a perfect system of tests anything of that nature would be unseemly in view of the limited number of students tested. The figures are offered for the sake of their suggestive value and in the hope that efforts toward refinement of method will be stimulated. The production of a highly developed system of psychological examinations for the scientific study of the college student will require long and painstaking research on the part of many investigators. By way of general remark it might be suggested that the tests of the future should be arranged with reference to the capacities of college students. The extensive use of mental tests with school children has resulted in a tendency toward the use of material and form better adapted to children than to adults. Other needs of a more specific nature have been pointed out throughout the text.

Whatever be the nature of the psychological tests that will be evolved in the future for use in this field, it is the method of utilizing them that is under consideration here. The function of psychological tests as here advocated is to furnish grounds for hypotheses regarding individual students and to supplement and give corroboration to other evidence of an interpretative nature. Such a rôle does not include predictive powers. It demands that the tests be regarded merely as instruments for one kind of measurement. In addition, it demands that the student be scien-

tifically measured in a variety of other relationships—physiological, academic, social and economic. Regarding the nature of the psychological measurement thus achieved, attention is called to the fact that this is not even defined. Emphatically it is not an absolute measure of mental ability. It is only relative—relative to the members of the college group. The present series does not even lay claim to completeness in comprehending the important phases of mental ability, though with the advances of future research such a goal may be approximated.

Such an aim for the application of psychological tests is clearly within the realm of possibility and in accord with the principles of scientific method. To one who has witnessed the application of these methods to the education of a group of able and willing young men and women for two years, it is evident that higher education may look with increasing hopes to psychological laboratories.

CHAPTER VII

Vocational Guidance and the College Student

The prevailing interest in vocational guidance and especially in the application of psychological methods to the problem makes it desirable that the methods hereinbefore described be examined as to their vocational significance.

Since President Eliot's[35] notable appeal for the maintenance of a life-work ideal before the college student, educators have begun to realize that much higher education misses point because of insufficient emphasis upon vocational ideals. Especially is the liberal arts course charged with ineffectiveness because it does not relate itself to the future life-work. Probably one cogent reason for this alleged ineffectiveness is that so many students taking the so-called "academic course" have no vocational aims. Under such circumstances the training given by the college must necessarily be that of a purely formal nature. Not knowing what are to be the conditions surrounding the individual in the future, the college must necessarily forbear relating its service to his life-work.

Granting that the college feels its obligation to consider the student in relation to his life-work it is apparent that it must modify the nature of its ministrations somewhat. If the student comes to the college unidentified with any life-work ideals it is the duty of the institution to help him secure a vision, or to use a mor~~p~~e popular term, to give him vocational guidance. It will be shown before the conclusion of this discussion that true vocational guidance means more than assistance in choosing an occupation; especially does the guidance demand of the college and university transcend this narrow interpretation of the term. Nevertheless, for immediate purposes this narrower standpoint will be taken and it will be seen that the college student is sadly in need of vocational guidance.

[35] Charles W. Eliot, The Value during Education of the Life Career Motive, Proceedings of the Nat. Educ. Assn. 1910. Pp. 133-141.

A few statistics[36] will show in some measure the conditions that exist in representative colleges and universities. An investigation was made by Dean L. C. Marshall among 744 undergraduates at the University of Chicago and 503 undergraduates at Ohio Wesleyan University which showed that of the men studied at the former institution, 74.6 had reached definite decisions and of those at the latter institution, 65.1. Of the women, the corresponding percentages were 64.6 at Chicago and 61.0 at Ohio Wesleyan. In an inquiry made by Dean Frederick P. Keppel of Columbia College among 800 graduates of Dartmouth and Columbia College for the years 1908-09-10, 519 replied, and of this number approximately 14 percent had not made a vocational decision at time of graduation. Of the 493 who had decided at time of the inquiry, 216 or 43.8 percent had decided before entering college and had not changed their minds since. Though figures supporting the statement are not given, it is hardly likely that more than two-thirds of the members of these two groups had definitely decided upon a vocation before entering college.

A similar investigation was made among students in the College of Science, Literature and Arts at the University of Minnesota which showed that 90 percent of the students had chosen a vocation at the time the inquiry was made—during the year 1911-12. Closer inspection of the 90 percent that had decided upon vocations shows that 67 per cent of them had decided while in high school or before. The other third had presumably decided at entrance to the university. This shows that in this particular group at least, more than a third of the students needed the direct aid of a vocational counselor at time of entrance to the university.

Even without these significant figures, probably most educators will agree that one-third of the college students enter college without definite vocational aims and unquestionably need direct vocational advice—advice which the institutions for higher education are not giving. Most of these students are considering various vocations in a perplexed state of mind. This was

[36] These statistics are not available in published form except those gathered by Dean Keppel which appeared in Educ. Rev. Vol. 40, pp. 433-9.

brought out in the studies at The University of Chicago and Ohio Wesleyan University. Of the men who had not yet reached decisions, 88.1 percent at the former institution and 78.4 at the latter institution, were *considering* definite vocations. Stated in another way, at the two institutions, only 3.0 percent and 7.5 percent respectively of the men neither had chosen nor were definitely considering vocations. This shows that the students are facing the problem and are in need of assistance in solving it.

But the need of this one third does not constitute the only justification for the establishment of organized vocational guidance in institutions for higher learning. To confine the process thus is to fail in interpreting its real meaning and the true scope of its need. Close inquiry would reveal that many of those students who have ostensibly chosen a life-work have based their decisions upon very slight knowledge either about the task or their own aptitude for it. Moreover with many of them the determination is by no means final. A very little questioning will bring out their own indecision about the matter. Nor is this unsettled state of mind or this tentativeness of choice a thing to be regretted. On the contrary it is a condition to be expected. As knowledge and experience increase it is natural and healthful that an individual should enlarge his vision and get new aspirations. However, in order to allow for these natural evolutions, the college should provide some means for guiding these aspirations. Granting that these two classes of students would be benefited by systematic vocational advice, one can see that even the remaining students who have hypothetically chosen vocations wisely and finally, need the services of a vocational counselor. With them, the advice will be directed toward a proper use of the curriculum, and this suggests the form which vocational guidance should assume in the college—the form of educational guidance. A proper introduction to the curriculum in itself constitutes real vocational guidance, for it will be based primarily upon the student's needs and capacities. It will call out all his resources and will seek compensations for his shortcomings.

Efficient guidance of students through college and university

requires first of all a flexible curriculum. This does not mean a wide-open system of electives; any abuse of that nature would be avoided by the second requirement that the course of every student be determined by an educational expert. The ordinary college student is incapable of merely choosing his own course. It must be done for him. There must be certain principles governing this selection of courses; a prefunctory signing of admission cards is by no means educational guidance. The selection must be made by one who has at his disposal a vast amount of information about the student. Information must be gathered that will show the capacities, the limitations and the past and present environment of the student, all of which must be related to the curriculum. This involves, as is readily seen, the kind of study described in the preceding chapters—the study that considers the student in all his relationships before presuming to shape his future.

Such a study of the individual conforms to the practices that should characterize true vocational guidance. The individual must be studied from all aspects—first physiologically. This should not stop with an examination of the single individual. His descent should be inquired into. Methods are being evolved in biological research for the prosecution of such inquiries and there is probability that considerable light may be thrown upon the powers of an individual from a study of his heredity. Psychological considerations come next, and it is around this phase of vocational guidance that some of the more picturesque and misleading notions cluster. Some of these will be discussed shortly. The social and economic status of the individual next demand careful consideration, and lastly, his interests should be examined. These lines of inquiry seem to comprehend the different factors which, combined, enter into the rationale of vocational guidance. It is possible that future developments will disclose other important relationships. In a concept that is so vaguely defined, and in a social order so volatile as the present, it is difficult to foresee all the complications that may arise. The thing to remember, however, is that vocational adjustment is never a matter for settlement along any one line.

Plans along the lines just indicated are being discussed with more or less clearness throughout the country and interest in the new ideal of vocational guidance is being rapidly aroused. It is forcing itself upon the attention of various classes of society—upon a misfitted and dissatisfied public who sees in it a possible solution of its individual vocational problems, upon industry which sees in it possible amelioration of numerous economic ills, and upon education which sees in it a means for the fulfilment of its highest responsibility, the fitting of the individual to cope with his environment.

As already indicated the ideas held regarding vocational guidance are quite various. People seem to be vaguely groping toward a working notion about it. On some points the way is clear. All see that vocational guidance has an informative function, that it requires the compilation of a large number of facts about occupations so that vocations may be chosen intelligent. Regarding the analysis of the individual, however, there is less clarity. The views that are held are extremely vague, and much is held that is erroneous. The fallacious assumptions involved should be pointed out if progress is to be made.

Perhaps the most serious error in the current notion of vocational guidance is indicated by the assumption that every individual is either "round" or "square"; that each individual is unalterably fitted for one kind of work but not fitted for any other kind. This mischievous doctrine must be overthrown before advance can be made in this field. Its refutation lies in the simple fact that many people can be trained to do several things equally well. In any group of individuals whose abilities are arranged according to the normal curve of distribution there are a few persons at the upper extreme who fall readily into the class of genius, and the line of their success is quite plainly indicated. There are a correspondingly small number of persons at the lower extreme whose deficiencies are so apparent that their vocational possibilities are likewise clearly limited. Between these extremes, however, are a large number—approximately 50%—who could be trained to do several things equally well. This makes it difficult to believe that an individual can rightly hold but one kind of position in the world of work.

A second misunderstanding concerns the use of psychological tests in vocational guidance.[37] In the popular mind this involves putting a person through a prescribed set of tests at perhaps one hour's sitting, and at the end concluding, "You should be a civil engineer," or something equally definite. In order to see the difficulties involved in such an expectation, some of the difficulties of all measurement should be pointed out. In the first place any single measurement must take into account the fact that power of achievement varies from day to day according to changes in weather, physiological and emotional conditions. This vitiates somewhat the reliability of single measurements, and any system of vocational tests that claims validity must make allowance for it. Furthermore, a single measurement will not show susceptibility to improvement. It cannot be ascertained from the results of a single test, to what extent an individual is capable of profiting by practice. Not only does the individual improve beyond the limits of his first attainments in a task, but also the degrees of improvability vary among individuals, and the arrangement of a preliminary series of measures does not bear a constant ratio to measures in successive trials. This is evidenced in widely different types of activity from simple processes to complex acts such as type-writing. It is evident, then, that any system of vocational tests that is to be reliable must guard against errors due to chance sampling and must provide for susceptibility to improvement. These objections do not constitute an insurmountable obstacle, since with sufficient research, it may be possible to devise a method whereby these factors may be weighted. Nevertheless they should be kept in mind as possible sources of error, and they emphasize the possibility that the vocational tests of the future may be more elaborate than at present supposed.

In the light of the foregoing it is apparent that if progress is to be made in vocational guidance it will be necessary to rid the mind of a belief in types, such as executive type, etc. The treacherousness of the type as a scientific concept has been repeatedly demonstrated by experiment, and as a vocational con-

[37] H. D. Kitson, Psychological Tests in Vocational Guidance. School Review, March, 1916. Pp. 207-14.

cept it falls down completely before the simple circumstance that many persons can be trained to do several things equally well.

It is frequently advocated that interest should be the ultimate criterion in giving vocational advice.[38] Various methods have been proposed for discovering interests. Questionnaires have been prepared in profusion, and various methods of studying expressions have been proposed such as studying involuntary reactions to various kinds of interesting stimuli. The great difficulty in being guided by interests is that many individuals possess a number of them, often of equal strength and of a conflicting nature. Furthermore they vary from time to time, especially during adolescent years. These individuals constitute the most difficult problem for vocational guidance and they can receive little help from a doctrine that uses interest as its chief criterion of aptitude.

The foregoing discussion points out some of the chief difficulties on the technical side that becloud the current notion about vocational guidance, and suggests the psychological reasons for its rejection or at least, modification. There are further considerations of a philosophical import that demand its revision. In the first place it maintains implicitly that in the grand cosmic scheme there is but one task that can be satisfactorily performed by a single individual; that the nature of this task is prearranged by the constitution of his psycho-physical organism, his social and economic milieu, etc. While this doctrine can not be completely invalidated on logical grounds, it is at least seriously open to question and should be carefully weighed before incorporating it into any concept so momentous as that of vocational guidance. According to this doctrine, the failures made by misfits are due to the fact that they did not find the right avenue for their talents. It implies that if a man finds his niche, success is assured; his efforts will always be in proportion to his stimulus and he is a mere puppet in the hands of fate. This doctrine, while embracing the conception of a beautifully well-ordered and harmonious universe, leaves out of account the factor of personal volition. It

[38] H. D. Kitson, Interest as a Criterion in Vocational Guidance. Educ. Rev. Nov. 1916, pp. 349-56.

tacitly assumes that if the methods of science be sufficiently refined one will be able to foretell with practical certainty the destiny of an individual. Such a program is feasible in astronomy where the course of fixed and soulless stars is concerned, but in the realm of human endeavor it is not likely to be of much avail as a working hypothesis. There is a further consideration that will have weight with all except those who are inalienably committed to a mechanistic conception of the universe. The current doctrine fails to make allowance for contingencies beyond the control of the individual. It displays a cock-sureness about the future that is not warranted by the course of human experience. The experience of every one will show occurrences which, even with the utmost care, could not have been provided for in advance. Whether one attributes these to Chance or to the "divinity that shapes our ends," they are a part of the universe and must be reckoned with. The glaring fault of the present doctrine may be said to be that it views the individual and society as two static entities. It says, "This youth should be a civil engineer," and looks upon him as one who should devote his entire life to that profession. Now as a matter of fact the individuals, and successful ones, too, who spend their lives in an unchanged line of work are not so numerous as one thinks. The individual constantly changes. He develops new interests, and when placed in new positions, frequently displays talents whose presence was previously unsuspected. Thus the civil engineer might by stress of circumstances develop into an excellent executive and lay aside his engineering duties. Still another person might reach that same executive position through an avenue other than that of the engineer. He might reach it through the sales avenue. There is considerable illumination to be thrown upon these problems by applying the concept of evolution to the vocational development of individuals, and in the careers of successful men it is quite easy to mark the stages of that evolutionary process, and to note how each change took advantage of the experience of former stages. But to suppose that science could foresee the course of that development and foretell its progress exactly were to place too great a strain upon the potentialities of scientific method and upon man's credulity.

Just as the relations of the individual should be regarded as dynamic in their nature, so also should society be regarded as dynamic. The kinds of work which the world requires are in constant change. Examples abound. For example, twenty years ago there were no such vocations as those of chauffer, aviator, etc. Twenty years hence there will be other new ones. The hide-bound method of assigning vocations would make no allowance for these developments, and hence breaks down.

It is admitted that all of these objectionable doctrines are not explicitly asserted as essential parts of vocational guidance as it is advocated by many persons, nevertheless it is apparent that the prevailing opinion is surely leaning in the directions mentioned. These criticisms are offered before the concept becomes crystallized in the hope that as it grows it will develop along healthy lines and not along lines that will bring it into discredit. In attempting to derive a formula that will stand the test of logic and experience, it seems necessary to regard the service of vocational guidance as chiefly *monitory* in character. It should eschew all pretensions to predictive power. It may properly only marshal facts and show to the individual the tendencies within himself. Given a wealth of information about occupations and accurate measurements of the individual in all his phases, one can only say at most: "If you enter this particular vocation you will be hampered in this or that respect and you will have this much in your favor. If you have sufficient determination you may rise above the handicaps and attain to some degree of success in the calling. Science, however, cannot place a tag upon you that will guarantee a safe journey over the road of least resistance to a goal of gratified ambition and unalloyed success."

Such an ideal as that just suggested, while not possessing the dramatic possibilities of the more picturesque "pigeon-hole" point of view, nevertheless seems more becoming as a working hypothesis. In the first place it recognizes the fact that the individual and society are in a dynamic state of interplay, not static. Second, it calls for nothing not within the range of scientific method. It is quite possible to conceive of a technique

that will enable one to give pointed advice without postulating any mysterious prescience. One who follows such an ideal will be free from the suspicion of charlatanism that might be urged against those who in order to tickle the ears of a credulous public, would make promises beyond the power of science to fulfill. This more modest ideal is also free from the errors of a fatalistic philosophy. It postulates no hypothetical "best way." It simply takes facts as it finds them and draws conclusions based on facts alone. One is forced to conclude that the former will never lead to a scientific attitude, while the latter adheres to scientific method. A further advantage that enhances the attractiveness of this *monitory* type of vocational guidance is that it relieves one individual from responsibility for another's successes or failures. A man of scientific mind revolts from the task of issuing ultimate fiats regarding the future such as are popularly demanded. Advice he will gladly give. Scientific measurements he will cheerfully make. Interpretation of these measures is his bounden duty. Further than this he can not go, and society should not ask more.

The evidence points to the untenability of the "pigeon-hole" theories of vocational guidance and to the adoption of a *monitory* theory. The relinquishment of the more pretentious ideal does not signify, however, that educators should lessen their zeal in the matter. On the contrary the adoption of this *monitory* conception should accelerate the progress of true vocational guidance. Society does not possess an elaborate technique for probing into the future but she does have abundant material for applying the fundamental principles of vocational guidance which consist in studying the individual in all his relationships. The informative function of vocational guidance can be partially performed by a proper utilization of census reports. Figures are compiled showing the distribution of different kinds of workers throughout the country, and these figures may be related to a number of other facts of distinct vocational significance, such as proportion to population, productivity of soil, climatic conditions, rate of mortality, scale of wages, etc. These statistics are available and only await their application to take a prominent place in the preparation of youth for vocations.

By way of analysis of the individual, there are methods available that might render excellent service if properly applied. Every institution that addresses itself to the preparation of men and women for life may take some steps toward vocational guidance. Ofttimes a bit of kindly advice from some member of the faculty who is not afraid to tell the truth, would save one from a misstep. But in addition, there should be one whose duty it is to secure all possible information about the student, academic, psychological, physiological, social and economic. On the basis of this many-sided view, many strengths and weaknesses will be brought out. The next step is to utilize the curriculum for the development within the student of those qualities which are needed for his success. Perhaps the college curriculum will have to be seriously modified in order to be thus used. Indeed, it is very likely that such will be the case. Not the least of the beneficial results from the movement of vocational guidance among colleges will be efforts to rationalize the curriculum and link it up more completely with the needs of society. All the benefits to be derived from organized vocational guidance can not be enumerated, but enough has been said to show that the educational institutions of the country would find it one of their most powerful agencies for the development of the individual to his highest capacity, and for the utilization of his powers by society.

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Whole vs. Part Methods in Motor Learning. A Comparative Study

BY

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WHOLE Vs. PART METHODS IN MOTOR LEARNING—A COMPARATIVE STUDY

CHAPTER I.

Nature of the Problem

One of the several problems of the general pedagogical-psychological field that warrants full analysis is the 'whole' vs. 'part' method of learning. Whole method procedure demands the continuous repetition of an entire body of material until the desired stage of mastery is attained. Part procedure requires an initial mastery of definite sections of the material and the final connection of these different sections in proper serial order. This 'whole'—'part' problem loomed large during the past decade, but interest in it seems to have waned, due no doubt to the acceptance of the experimental evidence as final. This evidence was first deduced from the learning of the Ebbinghaus nonsense syllables. This pioneer in the scientific study of memory set the problem. Meumann (11) presents his own work, planned to supplement the splendid efforts of Steffens (19). Ephrussi (4), Neumann (13), Pentschew (15) of the German laboratories, Larguier des Bancels (10) of the French, and Henderson (6), Kuhlmann (8), Lakenan (9), Pyle (17), Pyle and Snyder (18), Watt (21), and numerous other writers in English have investigated the problem. The statements of Meumann (11) may be summarized as typical, i.e., learning by parts becomes more disadvantageous the more the material is subdivided; conversely, the more closely the 'part' learning approximates to 'whole' learning, the more rapidly and certainly is the task accomplished. The learning advantage for the 'whole' method is even greater with meaningful material than with the nonsensical. The superiority of the 'whole' method is manifested by fewer repetitions being required for mastery, more correct formations of associations, and more permanent retention. These results hold for the adult and also for the

child, as soon as the latter becomes aware of the advantages of the 'whole' method. These findings are likewise true for material not constituting a coherent whole. There are possible mediating procedures of the 'whole' method, such as brief resting pauses in the forward directed method or the temporary delay upon parts of obvious difficulty.

The material presented and the evidence reviewed lead to the acceptance of the 'whole' method as the more efficient in the learning of nonsense material and poetry of short lengths. For longer lengths, Pyle and Snyder (18) verified the results for poetic material of 240 lines and Lakenan (9) for prose of 300 words.

Following Steffens, Meumann, Pentschew, and others, the following causes of waste in part learning are, perhaps, suggested, though none of these have been subjected to laboratory testing.

(a) Learning of transitions between units (b) Learning of backward directed associations. (c) Break up of helpful mediate associations and the disturbance of the absolute position assigned to each item during the learning of its own part or section. (d) Lack of uniformity in distribution of the learning. (e) Loss of the aid of logical coherence with sense material.

The above brief survey points to the wide interest the problem has attained and to the strong agreement of the experimental findings. But the interest has not been carried over into the motor field and very few references have been made to the conditions as they would appear in definite class-room situations. Parker (14) is a notable exception. In his recent "Methods of Teaching in High Schools," he suggests that, in the school activities of the gymnasium and shop, in dancing, in musical technique, in the pronunciation of a foreign language, etc., motor control may in some instances possibly be hastened if attention is directed toward the elementary movements involved. Excluding this bare suggestion, the question of the unit ('part') method as opposed to 'whole' learning of the motor act seems to have been disregarded by the experimental

psychologist. Dearborn, Ordahl, Richardson, Swift, Freeman, Colvin, Book, Bryan and Harter, Leuba and Hyde, Ruger, and Thorndike may be mentioned as having either overlooked the problem or else considered the evidence in rote and logical learning so conclusive as to warrant analysis of the parallel motor situation relatively useless.

Summarizing, the following points are descriptive of the work done upon the problem of 'whole' vs. 'part' learning: (a) It has been confined to logical and rote material. (b) Humans have been tested but not animals. (c) The pure 'part' method has been investigated but practically no modifications of it. (d) Greater economy obtains with the 'whole' method. (e) Several proposed explanations of the waste in 'part' learning have been offered, but none of these has been tested under controlled conditions.

The concern of this research, then, may be stated in certain definite propositions:

(1) To see whether the 'whole' and 'part' findings in rote and logical learning hold for sensory-motor, adaptive problems;

(2) To determine whether these laws hold for animals as well as humans when learning conditions are comparable;

(3) To test out certain hypotheses and determine which factors are causative for economy or waste in these methods of learning;

(4) To devise such modifications or combinations as may be better than either of the above methods;

(5) To draw such conclusions from the data secured as may have heuristic and practical values in enforcing or modifying learning conditions imposed upon the school child.

The above program seems to the writer to be timely. The poverty of knowledge of the motor field is obvious. As regards the attempt to determine universality of methods, the comparative psychologist has likewise been dilatory. Barring the research of Hunter (7) upon the Delayed Reaction, the literature fails to show a thoroughgoing attempt to elicit data for humans and animals secured under identical conditions. If the sole excuse for comparative study is to secure information that

will promote the prediction and control of human behavior rather than to gather facts of animal behavior that have a value "in and for themselves", then this research certainly seems opportune. Furthermore, this research makes a thoroughgoing attempt to reduce to a measurable level such obscure terms as the strength of backward directed associations, the learning of transitions between units, etc. Finally, the need for bettering existing methods of learning is always urgent. No doubt criticism may be raised regarding the details of the research. But the writer hopes that his methods will prove stimulating and suggestive to other investigators and that experimentation in an exact and truly comparative sense may be carried out by them. Until the identity of learning conditions is established, any talk of relative degrees of intelligence between different life forms, universality of behavior, etc., seems little short of academic.

In the search for a motor problem where conditions of learning might be as nearly identical as possible for the human and the animal, the maze was selected. The researches of Small, Watson, Carr, Kinnaman, Porter, Yerkes, Hicks, Vincent, Bogardus and Henke, and numerous additional papers, of scarcely less magnitude have served to make the maze problem well nigh commonplace with the psychological experimenter. Perrin (16) continued the work of the Chicago Laboratory with pencil mazes, but the comparative possibilities of his work were not followed up[1] A description of the rat and pencil mazes used by the writer and an analysis of the method of experimentation bring out the comparability of conditions in the present research.

Apparatus and Procedure

A maze of special design was constructed, the details being determined solely for their adaptability to the 'whole' and 'part' learning methods. This maze "A" (page 70) was square,

[1] Since the above was written, Dr. Perrin's (5) article upon the human and child maze reactions has appeared. It will prove interesting to the comparative psychologist to have this valuable experimentation duplicated with the rat.

with a food-box 8″-8″ in the center. The maze consisted of four independent sections, each having its own entrance and exit into the food-box. A distributing gallery around the food-box made it possible by the removal of the panels to learn the sections in any order and to connect them as desired without changing the general exit into the common food-box. Several of the connections are discussed in detail in subsequent passages. The dotted lines (Fig. I, p. 70) show the position of doors and removable panels. It is at once obvious that the sectional arrangements, the conditions of entering and leaving each section, the absolute simplicity of throwing the various sections into a larger motor situation, etc., render this design of immense value for the problem under investigation. The doors and panels were of galvanized iron and worked vertically in slotted brass posts. The posts were securely screwed through the floor of the maze to a 16″-16″ metal plate fastened under the maze floor. The passageways were 4″ in width and height. The partitions were made of ⅜″ stock. Each section contained three cul de sacs, each being 12″ in depth. The final one in each section (immediately preceding the turn to the food-box) was the same in general position for all sections. The remainder of the blinds were differently placed, furnishing four distinct maze patterns. The true pathway for each section was of constant length, 100″. This equality of the four sections in point of number of possible errors and length of the true pathway is partially comparable to logical memory conditions (where verses to be learned are of the same length) and rote material (where the length of the series and its parts are easily controllable). When the parts were thrown together, the total distance represented in the twelve cul de sacs was 48″, with 400″ in the true pathway. The interior walls, panels and doors were painted black. Covering was by four glass frames, the food-box being left open. Sliding panels were attached to the right wall of errors number 3, 6, 9, 12. A rod extended through the outer wall of the maze box and, when this was pulled, it closed the passageway and prevented the return of the rat over the section just traversed.

Such a device made it easy to test the influence of the returns in maze learning.

A second maze (Maze B) was one used by Bogardus and Henke (1). This was used only to verify the results obtained for one phase of the experimentation upon Maze A, namely, the influence of preventing returns. It was unsuited in design for further use. It contained double section alleys, these totalling thirteen single sections. Sliding doors were arranged for blocking returns at the end of sections marked b, d, and h. Consequently, four distinct maze areas are learned but these do not approach equality in number of errors, length of pathway, etc., as in Maze A.

The human mazes duplicated exactly the pattern of the ones just described. They were constructed out of solid brass. The walls were made equal in thickness to the passage-ways, namely, .7 cm. Maze A had cul de sacs 4 cm. in length. The true pathway covered 30 cm. in each section. The entrances, exits, blocking panels, etc, were solid brass plugs, each equipped with a small metal post and fitted into a hole drilled in the maze floor. The plugs were carefully adjusted and never presented rough edges. To the tactual sense, they were parts of the regular maze wall. By adjustment, the sections could be learned in any order and the run modified in the same ways as described above for the animal mazes. Sections could be eliminated without disturbing the general exit into the common open place. Maze B was constructed along similar lines, though without any detail of sectional complexity, since its utility was very limited.

Each brass maze was laid flat on the table when in use and any movement during the testing act was prevented by restraining strips tacked around it. The entire table was covered with a black cloth hood. The subject could move his arm freely under the hood, so that his learning of the maze was unobstructed. His only handicap consisted in being deprived of vision. The hood was open toward the observer, so that the learning efforts of each trial could be observed and recorded.

Animals selected for the experimentation were white rats.

They were secured mainly from the local dealers as needed. Some few groups were bred in the laboratory. No strain selection was attempted. For all the groups, training began at the age of eight or nine weeks. The rats were caged in groups varying from five to seven for a cage and not segregated. The cages were placed on racks around the walls of a 12′ by 12′ room and were never moved from position during the learning period. The rats were fed in the food-box for a period of ten days before the tuition was begun. They were allowed to run at will over the glass top of the maze. They became accustomed to the feeding environment and to human handling. Food consisted of a bread and milk diet, each group being fed in the food-box seven minutes per day following the completion of the day's run. Also, each rat was allowed a nibble of food upon reaching the food-box after each run. The cages were cleaned once per week while the group was feeding. Any disturbances due to changes in bedding, etc., were hereby given opportunity for subsiding during the twenty-four hours intervening before the next trial. During the day, shades to three windows were raised for sanitary reasons. These were invariably drawn when the experimenter entered the room for the day's testing and electric lights were switched on, one occupying the center of the ceiling and directly above the maze, the other a drop light six feet to the rear of the main maze entrance. All testing was done by electric light. One hundred and seventy-seven rats were trained, ninety-one male and eighty-six female.

The human subjects were university students from the writer's classes in Introductory Psychology. Their college classification called for sophomore standing or higher. Seventy-five percent were sophomores. There were fifty-three men and fifty-nine women used for the testing. The testing groups numbered six. Each student reported privately for his test at a period kept constant from day to day. Testing continued at this regular period each day (barring Sunday) until the maze was mastered. No testing was permitted with visitors present. The testing was done in an annex to the experimenter's

office. Constant conditions of lighting, furniture arrangement and quietness were maintained. It is here in order to express thanks to the students for their long-continued and punctual observance of the testing conditions. Without their faithfulness, the results would be vitiated.

Regarding the method of testing, each rat was given one run per day in the maze for four days. Following this, two runs were given in succession per day until four out of five successive runs were without errors. Learning was then considered finished. Time was recorded with a stop-watch from the time the rat turned from the entrance door until he emerged into the food-box. Errors of three types were listed separately. (1) Cul de sacs entered while the rat was going forward. These are called Type A errors. Entrance into a cul de sac was considered accomplished whenever the body was squarely oriented in the error pathway. (2) Cul de sacs entered while the rat was returning toward the entrance, i.e., blind alley errors due to the retracing. These are called Type B errors. (3) Retracing over the true pathway. These are called Type C errors. Such are scored when the rat is returning toward the entrance. Each short section of the return pathway traversed constitutes one such error. During the runs the experimenter was seated back of the main entrance and retained this position, irrespective of the complexity of a particular learning method. The maze box was so constructed that the rat could be placed in the various entry-ways without causing any change in the experimenter's position.

The human subjects were given the same number of runs per day as the rats. The criteria for mastery, scoring of data, etc., were likewise identical. The results of each trial were listed during the run (or immediately following in the case of hurried, almost perfect runs). When the subject was ready for the first run, the experimenter lifted the hand on to the maze area, fixing the stylus in the required locality. The following instructions were then given: "You are now on a surface that has a pathway in various directions. Explore the area, being careful to keep the pointer in the groove and

not allowing it to become dislodged—so! Continue to explore the area until I tell you to stop." No description of this or of any maze was given. No directions as to the types of errors, their avoidances, or striving for speed were given at any time. The subjects were chosen primarily because of their total unfamiliarity with the maze problem. Only after the runner had explored the entire surface and reached the open area, did the experimenter say, "You are now in a large, square area—so! That is called home. You must learn to reach home in the most economical fashion."

It is obvious that the human is forced under these conditions to rely upon contact values for the detection of blinds and the gaining of a sense of direction. Deprived of vision, he is 'sizing' up the novel situation as the rat has been shown to do, a fact ably demonstrated by Watson, Small, Carr and others. In the same stumbling, trial and error fashion he learns the concrete meaning of blind alleys, returning to a closed entrance and the final position that means success. No attempt is here made to state that the mental processes involved in the mastery of the maze situation are identical for the rat and the human. It is maintained, however, that the two have been forced to determine the nature of a situation regarding which they were equally in ignorance and to rely upon the same sensory avenues for data gathering. The satisfying of these conditions is a prerequisite for any comparative study.

CHAPTER II

Comparison of the 'Whole' and 'Part' Methods With Returns Permitted

The introductory chapter has brought out the fact that the maze problem was the one chosen for testing the 'whole' and 'part' procedures. It has been shown that this choice makes possible the establishment of identical conditions of learning for the rat and the human. The identity of the maze problems and the duplication of testing conditions for the rats and humans have been fully set forth. The specially designed mazes have been described at length and their adaptability for testing the 'whole' and 'part' methods commented upon. The present chapter shows how groups of rats and humans were taught the problem by these different methods. Each group is treated separately and its learning behavior and records are displayed below.

(a) Utilizing Maze A, a group of twelve rate, five males and seven females was used to establish results for 'whole' method learning. The behavior of these rats in learning the maze was different in no way from the descriptions generally given. The results of this regular method of maze learning appear in Table II. See page 72.

(b) For learning Maze A by the 'part' method, a group of nine rats, four males and five females was used. The rats were trained in Section I until mastery was attained. As soon as the individual rat had reached this stage, he was transferred to Section II, using of course entrance II and exit II. It has been shown in Chapter I how the learning of a section did not involve any of the other sections, since each section has an independent entrance and exit to the food-box. Upon mastery of this Section, tuition in the remaining units was successively carried out. The behavior in learning the four distinct sections presented no peculiarities, except the readiness with which the

rat began the learning of each new problem. The general hesitancy of attitude was lacking after Section I had been learned. As soon as the mastery of the four units was attained, the separating panels were removed and the rat started at entrance I. The difference in behavior now became marked and was characteristic for the entire group. Starting off at full speed and with almost uniform perfection in Section I, the rat would come suddenly to a halt at the closed door of exit I. Sometimes he would dart back through Section I to the entrance and would return full tilt. A hurried run into Section II produced the same result at exit II. Hereupon the rat's reaction generally went to pieces. Occasionally he might run perfectly into Section II, check his speed, stop, and then return the entire maze length. Retracing, entering blind alleys long since eliminated, pausing, cautionsly exploring the various Sections were characteristic features. Frequent complete returns were made. Occasionally a fresh start and a rapid run would suffice to carry the rat through the entire course. But each rat of the group behaved uniformly respecting the inability to connect the serially learned units, the enormous time lost in retracing and exploring, and the speed of motion. Nor did this confusion subside after the first successful act of connection, as is shown from the data tabulated below. Table I gives the number of trials and the time required for each Section and for the connection of these, together with the errors. Table II gives these results in comparison with the group learning by the 'whole' method. (See page 72.)

These numerical data show an advantage (10%) in the number of learning trials for the whole method, but this is offset by an enormous expenditure of time (118%) and errors made (9.5%)[1] An inspection of the types of errors reveals that the

[1] The attention of the reader is called to the case of a rat of the 'part' learning group, whose records are excluded from the data presented. This rat learned the different units in normal fashion but was unable to connect these. For the first trial, he ran perfectly into Section II, thence retraced until the entrance to Section I was reached. Here he sat for one hour, whereupon he was removed. For this run, he scored no forward going blind alleys. Two were made on the return and twenty-seven re

high number was due to retracing both the true pathway and the cul de sacs. These are more numerous in each case for the 'whole' method (139 vs. 108 for the retrace errors and 24 vs. 17 for the retracing cul de sacs). Much of the great time expenditure in 'whole' method learning occurs in this retracing activity. Consequently, it points out one disadvantage of using the 'whole' method for learning the maze. The problem immediately emerges whether such repetition and time expenditure due to retracing are advantageous. This is seriously called into question, since the only favorable score of the 'whole' method is in the scant saving of 10% of trials. The utility of the returning effort will be investigated in Chapter III.

(c) With the human experimentation in 'whole' method learning, the behavior was identical with the general type as described by Perrin. It agreed also with that of the rats. The early trials accumulated errors of all types. Much time was expended in apparently useless movements into blinds and repeated returning to the entrance. Often the subject would pause as if for reflection and then attack the problem with renewed zeal. See Table IV (page 72) for the results of this test.

(d) In the 'part' learning, the subject was never told when he was set to work upon a new section, yet he seemed to detect the change very quickly. As in the case of the rat, the human worked hard to master each new problem. When the four units were learned, the act of connection gave the same difficulty as was shown by the rats. Some subjects seemed to have a strong determination to go abead but their control over the situation invariably failed.[2] The quickest subject to connect

traces. On the next day he started out rapidly, ran without error into Section II, returned without error to the entrance, where he sat until removed one hour later. He was not run again. He was the single rat of the entire group unable to connect the units.

[2] It is clear that the human subject knew no more of the nature of his task than the rat. He had not been informed that he was to connect four sections that had been learned as units. It was part of his problem to discover this, just as it was with the rat. The writer cannot say whether such previous information would have modified his learning results. Such previous instruction would certainly have rendered comparison with the rat records impossible.

the units required six trials, while the slowest required forty-seven. The records of this experiment are listed in Table III, and compared in Table IV with the 'whole' method records.

This comparison of human records shows an enormous advantage of the 'whole' method and this advantage applies to all the measuring criteria. There is a superiority of 47% for both total errors and time and 48% saving for number of trials. Likewise, this advantage is equitably distributed for all types of errors, since there is a substantial saving for each type when learning is by the 'whole' method.

Comparing the records of the rats and humans (Table V, p. 73), we find agreement in that the 'whole' method brings final success with fewer trials, though with greater percentage of gain for the humans. It is seen that the rats learn their problem with a saving of time and errors by the 'part' method, as opposed to the humans succeeding best by the 'whole'. Yet the 'whole' method is also more efficient for the rats, if the forward going cul de sacs (Type A errors) are made the criterion of measurement. If the retracings were eliminated from the records, the 'whole' method would prove superior in all respects, both for rats and humans. (This shows again the necessity of testing the influence of the returning, especially with the rats). In absolute terms, the humans learn the problem with fewer trials and less time than the rats, both for 'whole' and 'part' learning methods. They accumulate more errors than the rats when the 'part' method is employed, fewer errors with the 'whole' method. This high error accumulation in 'part' learning is assignable mainly to the connecting of the parts. Rats and humans agree in finding this connecting process very difficult, but the humans here require more trials and accumulate more errors of all types, especially of the retracing variety (Type C).

The results of this 'whole' and 'part' testing may be summarized as follows:

(1) Rats. Mastery is attained with a slightly less number of trials when learning is by the 'whole' method. Such learning accumulates more errors and requires a much greater time

expenditure. The errors in excess are not cul de sacs entered while going forward (Type A) but those due to retracing (Type C) and the cul de sacs made possible by this (Type B).

(2) Humans. Mastery is attained with fewer trials, less time expenditure and fewer errors of all types when learning is by the 'whole' method.

It is apparent that additional testing is required to determine the influence of the returning tendency. This alone prevented the 'whole' method from proving more efficient in all cases. If the returns are not counted in the records, it has been shown that the 'whole' method would be better for the rats in all respects, as it had proved with the humans. But it is not justifiable to exclude these returns arbitrarily. Rather, a test situation must be prepared where no more returning is allowed in 'whole' method procedure than in 'part' learning. This is the problem of Chapter III.

CHAPTER III

Influence of the Prevention of Returns

The experimentation reported in Chapter II made clear that the 'whole' method invariably proves superior with the humans and likewise with the rats, except when comparison is with reference to the great number of retrace errors accumulated by the latter. It was shown that the rats probably have a greater tendency to return than the humans, and that this tendency is no doubt exaggerated in the 'whole' method procedure. Logically, it is a question whether these returns are causal parts of the learning process or merely incidental by-products. If they are assisting in the mastery of the problem, they must be counted, both for rats and humans and in both learning methods. It might appear, consequently, that their relative advantage would be different not only for the different methods but also for the rats and humans. It was pointed out that the rats accumulated a high proportion of these errors in 'whole' method learning but that the humans did not. On the other hand, if these errors are shown to be relatively useless, they should not be counted in any case for either animals or humans. The problem of this chapter is to test the influence of these returns.

The equipment of Maze A with sliding panels for the prevention of returns has been described in the introduction. It is, of course, obvious that all returning is not prevented. It is not feasible to prevent all retracing. For our comparative purposes, it is necessary to restrict the returns in 'whole' procedure to the same number possible in 'part' method learning. This demands preventing the return into a definite unit section as soon as this section has been traversed. This effectively divides the whole maze into the four units established for 'part' learning. It renders the amount of returning in the 'whole' method plan practically the same as naturally occurs in the 'part' method.

It makes possible a comparison of the two methods with the same degree of returning. This is exactly the condition desired.

A group of nine rats, four males and five females, was used in this test. As soon as the rat had reached the closed exit to Section I, the return panel was noiselessly pulled. By quietly stepping to the opposite side of the maze the operator was enabled to close the blocks to Sections II and III without distracting the animal from his task of exploration. Often the animals would return to the closed passageway, but the finding of this blocked never resulted in fright or the cessation of the exploring activity. At no time did this group manifest the confusion and random expenditure of energy so typical of the previously described groups. The results of this experiment are arranged for comparative purposes in Table VI.

The returns in the human experimentation were prevented by inserting the tip of a long handled rubber block. This was constructed to fit the pathway completely. It was so held by the experimenter as to prevent any motion if the subject re-explored the area. Because of the general maze direction it was possible to block the returns without getting in the way of the subject. For the first few trials the subject was confused at his inability to return. All knowledge that his pathway had been blocked seemed lacking and he assigned his inability to his own carelessness (see Table VI, page 73).

Inspecting the data of Table VI, its remarkable uniformity of results is manifest. For both animal and human learning, prevention of returns increases the number of trials required for complete mastery, but at an enormous saving of time and errors. So far as regards time and errors, the greater amount of saving is for the rats (151% vs. 18% for time, 95% vs. 56% for errors). With trials, there is 10% vs. 29% increase in number for the humans. Considering the high savings in time and errors and the relatively small loss in number of trials, efficiency as between these types of 'whole' methods rests overwhelmingly with the prevention of returns.

The study of the influence of the returns as a factor in learning was continued with Maze B. This experiment in-

volves the double section alley as contrasted with the single section alley of Maze A. Fourteen rats were used for the unobstructed learning, thirteen for learning with returns blocked. The entire groups were given fifty runs upon the maze, and the records of all the rats for the entire period are combined. These are summarized in Table VII, p. 73. They show for the groups a large amount of saving, both for time and errors.

By the end of the tuition period, 64% of the unrestricted group had mastered the maze but only 54% of the group where returns were prevented. The records of these somewhat quicker learners are abstracted and appear in Table VIII. They show the same time and error saving as do the entire group records. While a larger percentage of the unrestricted had mastered the maze within the alloted period, the average number of trials required is slightly higher. This latter fact is at variance with the parallel results in Maze A. There is no reason to assign this to the difference in type of alleys of the two mazes, or to attempt any explanation, since the results are for a picked group. If, however, the results for the humans upon the same double section maze (where learning was continued until mastery was attained), should contradict the conclusions drawn from the work on Maze A, the question of double section alleys might be raised. Otherwise, it might be concluded that the full completion of the training would produce the results in increased number of trials for obstructed learning as previously stated for Maze A.

Turning to the human learning of Maze B, here again it is found that the entire group masters the problem with less runs when freedom is allowed but that more errors are amassed (Table VIII, p. 73). The restricted group fails slightly to maintain the time advantages generally secured (2.7% decrease), but this and the extraordinarily high number of trials are traceable no doubt to two students of the restricted group, who required 79 and 87 runs respectively for mastery.[1]

[1] The daily records of these two fail to show a continuous fixation of specific errors but rather a general inability to secure a uniform record from day to day. The extreme length of their learning series increased a native tendency to nervousness.

From the data of human and rat learning, both for Mazes A and B and not only for complete mastery but also for a limited number of trials, it seems correct to conclude that prevention of returns increases slightly the number of trials required for mastery but this is accomplished by an enormous saving in time and accumulated errors.

It cannot now be said that the retracing plays no part in the final mastery of the maze situation. Such an answer will depend finally upon maze records where returns are absolutely blocked. The writer is planning the details of such an experiment. He now has in preparation a detailed analysis of the learning of the maze with and without prevention of returns. Here there will be an attempt made to determine the exact value of the retrace error and the retrace cul de sac as factors in mastery. It appears almost conclusive from present data that the retracing and entering into blind alleys, made possible by this retracing are practically useless items. Graphs for the rats learning Maze A show that retracing is a negligible factor long before the first half of the number of required trials has been made; that the return cul de sacs cease to play any part after the first two-fifths of the trials in the case of unobstructed learning and after the first fifth for obstructed; that the forward directed errors are almost identical in number and in distribution throughout the entire learning period and that they and they almost solely determine the learning curve for the last half of the tuition period. (See graphs I to IV). This seems to indicate that the beginning trials are very wasteful when waste is permitted; that the maze is never mastered until the rat finally settles down to the difficult task of forward elimination of errors; that the early trials do not measure learning but primarily the extravagant and useless expenditure of energy. This would argue that the retracing is mainly of no efficiency in learning and hence should be disregarded. If the retracings are eliminated from the records discussed in Chapter II, it is certain that the 'whole' method ranks superior in every possible respect. It was insisted upon in the earlier discussion that the retracings are

the only factors contributing to the high error scores of the 'whole' method procedure, and that such a condition maintained only with the rats. This view is certainly strengthened by the findings of the present chapter. The obvious implications of this are two. (1) It seems fair to question whether the customary practice of including the retracing in the measurement of maze learning is justified. (2) Considerable light is shed upon the advantage of the complete forward direction of effort throughout the entire learning period.

Furthermore, this evidence leads the writer to question the reliability of viewpoint basic to the recent controversial literature regarding the order of the elimination of errors in maze learning. It seems important that the investigators should take into account the big question regarding the influence of the prevention of the returns before any conclusions are drawn in reference to a general eliminative tendency. Again, if true learning is not to be measured by total errors (as suggested above), should the measuring of the alleged eliminative tendency be begun until the stage of aggressive, forward directed learning is reached? This monograph waives any discussion of the order of error elimination, but the topic will be discussed at a later period.

Having tested out the influences of the partial prevention of returns and found that such procedure produces an enormous saving in time and errors, it is in order to compare these results with the statistics of 'part' learning as presented in Chapter II. See Table IX, p. 74, for the data. For both rats and humans, the 'whole' method is strikingly superior. Any question of approximate value for the two methods would rest upon the like number of trials for the rat learning (30 trials for both 'whole' and 'part'). But the evaluation of the two methods as equally efficient is totally unwarranted in the face of the overwhelming decrease of time (13%) and errors (44%). With the humans, this decrease is 26, 126 and 193% respectively for number of trials, time and errors. It is certain, then, that for both rats and humans the 'whole' method in motor learning is to be preferred to the 'part' method, provided unlimited

returning is prevented, or if not prevented, that the return errors are excluded from the data being compared.

The results of testing the prevention of the returns in 'whole' method procedure may be summarized as follows:

(1) The number of trials for mastery is slightly increased, both for rats and humans.

(2) There is a very marked saving in time for mastery and the number of errors is greatly reduced.

(3) The saving in time and the avoidance of errors are assignable directly to the prevention of the retracing.

(4) The forward going cul de sac errors (Type A) are almost constant in number with each method for the same divisions of the learning period.

(5) The retracing is largely useless and should probably be disregarded.

(6) The rat's tendency to retrace is stronger than the human's, but the prevention of returns affects the learning of both in identical fashion.

(7) 'Whole' method learning is more efficient than 'part' learning for both rats and humans, provided no more retracing is allowed than is possible in 'part" procedure.

CHAPTER IV

Elements of Waste in 'Part' Learning

The preceding chapter has shown that the 'whole' method of motor learning proves superior in all cases, provided no more retracing is allowed than is present in 'part' learning. It is conclusive that the 'part' method fails to secure success with fewer trials, less time consumption, and the accumulation of fewer errors. Naturally, it is necessary to determine the exact factors that create such a condition. Specifically, this means to seek out experimentally the elements of weakness in the 'part' method. Investigators of the methods in the rote and logical fields came to agreement regarding the causes contributing toward making the 'whole' method universally superior. These proposed explanations have been stated in the opening chapter (see p. 2). Two comments need to be made. These proposed causative factors have never been tested in motor learning, nor have they ever been subjected to measurement under carefully controlled laboratory conditions. The aim of this chapter is to test various *a priori* hypotheses and hereby secure a statistical evaluation of their validity. Several of those proposed for rote and logical learning are handled. However, several distinctly new conditioning factors are tested, not only because of their immediate connection with the maze act but also because of their logical reference to learning in general.

(a) Loss due to negative transfer in the learning of the motor units.

Until determined to the contrary, it may be argued that learning one section (one motor unit) exerts an unfavorable influence upon the mastery of succeeding units. While transfer in motor learning has often been investigated, there are no results that show conclusively whether such transfer—either positive or negative—continues unchecked in operation for sev-

eral successive problems. This influence may be so great as to cause an enormous expenditure of learning effort upon the subsequent maze sections. This harmful negative transfer may be the chief factor contributing to the accumulation of the numerous errors in 'part' learning. Reference to Tables I and III shows that this situation cannot be ignored.

Control groups, numbering six for both rats and humans, were taught as a single problem either Part II, III or IV of Maze A. These records denote the normal time required for the mastery of each unit when the learner is free from the influence of a previous learning act. A comparison of these with the records of the groups learning all the four units (the 'part' learners discussed in Chapter II) reveals that successive learning is rendered far easier by previous related activity (See Tables X, XI, p. 74). This positive transfer can be roughly estimated by bringing together our measurements of learning in the formula $T = \frac{(t-t')\ (s-s')\ (e-e')}{t.s.e.}$, where t, s, and e represent the number of trials, time in seconds and errors respectively for the case of original learning and t', s', and e' for the parallelled transfer conditions. The formula thus stated relates the amount of saving to the original learning conditions. Employing it, there is found positive transfer of 43, 47 and 9% respectively for Sects. II, III, and IV in the rat situation, and 2.3, 46 and 70% for the human.[1] Instead of finding an element of waste in 'part' learning, there has been revealed one of its fundamental advantages. Therefore, transfer as an element of waste in 'part' learning of this maze type must be

[1] The percentage of transfer increases with the human throughout the learning of the four mazes. The rat percentage for the fourth problem is relatively a marked decline. This is due to a single rat of the 'part'-learning group requiring 47 runs to eliminate a single error. This raised the group average so high as to allow only a factor of ¼ run to function for the number of trial gain in the formulaic estimation. It is an interesting problem to determine whether transfer should be increasingly favorable in successive mazes numbering more than four and to what limits. The writer believes it obtains certainly for four simple mazes and probably beyond the number. Many factors naturally enter in.

rejected, for transfer is strongly positive and advantageous.
(b) Loss due to disintegration through time.

Considering that the learning effort was distributed from day to day and that the mastery of the individual units involved relatively long periods of time, it appears logical to assign a good part of the great loss in 'part' learning to a mere forgetting of the earlier learned pathways. By averaging the number of days elapsing between the learning of Sections I, II, and III, and the return to these in the final act of connection, there is found a very appreciable time interval. This is fifteen, eleven, and four days for section I, II, and III respectively with the rats and thirteen, eight, and five respectively for the humans.

Different groups of untrained rats were taught a single unit and then allowed to rest for the required time interval. During this interval, the rats were placed daily upon the maze top and allowed to run for one minute before food was placed in the food-box. This furnished approximately the same amount of daily activity upon the maze and gave the rat the exercise demanded to preserve good bodily conditions. Cramped cage conditions necessitate this. At the end of the interval, the rats were retrained upon their respective sections. This was continued until the mastery criterion of four successful runs was satisfied. Hence, the disintegration through time is measured by the relearning expenditure. Human groups were excused from reporting to the laboratory for the appropriate interval. Their loss due to time is likewise measured by the relearning method. Tables XV, XVI, pp. 75-6, present these data. The disintegration is notably small. In fact, nearly all the group members were perfect in retention and the relearning effort was mainly expended by a single rat and some human subjects that had originally learned their problem very hurriedly.[2] It is certain, therefore, that disintegration through time must be disregarded as a waste element in motor 'part' learning.[3]

[2] This was especially true with Rat Number 4 of the Section II group. His original learning required but one trial, with two errors, and 487 seconds. His relearning required seven trials, eleven errors, and forty seconds. This suggests not only the question of relationship between speed of

(c) Loss due to retro-active inhibition.

It has been shown above that learning one motor unit is favorable for mastering subsequent ones and that a unit will not disintegrate during a limited time interval, provided only one such unit has been learned. But are the conditions reversed when the rat is taught several such units? Specifically, do the learning efforts expended in mastering Sections II, III, & IV, impair the ability of running I, II, III, and even the last mastered, IV? The influence of the interval of time under such conditions may logically be disregarded (see section above) but it is mandatory that the control over each sectional pathway be carefully tested. If this control has been broken up by subsequent learning activity, it is obvious that herein rests the explanation for much of the inability to connect the units in the final motor series.

An entirely new control group of rats was trained upon the

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learning and accuracy in retention but also whether the maze has ever been learned until the rat has taken time to work it out thoroughly,—either in the original learning or relearning process. There is fundamental difference between knowing how to steer by the unexplored areas (cul de sacs never entered) and knowing the character, depth, and position of these. The writer has never had a rat that did not work out the maze completely, either in the original or relearning situation. Considerable data relative to the learning time and retention accuracy will be published at a later period.

[3] This paper has waived detailed discussion of the question of retention, though the writer heartily agrees that one measure of the efficiency of a learning method is the strength of retention (as Meumann had shown in the case of rote learning). Group records have been accumulated for 'whole' method learners, with and without the prevention of returns, and for eight, seven, and five weeks. No big differences seem to appear for the same time intervals. The accuracy is very high, the loss being in the time of the first several runs in retesting. This is not due to exploration of cul de sacs or retracing but to slow and cautious rate of forward progress. Records for 'whole' and 'part' learners, where there was *progressive* retesting for 1, 2, 3, and 4 week intervals between the retesting (not the original learning) seem again to reveal strong retention, but with a probable time value in favor of the 'whole' learners. This is to be expected, since the final four perfect runs of 'part' learners in the original learning act are invariably slower than for the 'whole' learners. However, the writer regards the retention question in its relation to original learning methods as being practically unattacked in this motor realm.

four motor units, following the exact procedure laid down for the 'part' group. As soon as the individual rat had mastered the final unit, Section IV, he was retrained upon Section I. Such retraining was kept up until the mastery criterion of four successful runs was satisfied. By this relearning method, therefore, the experimenter was enabled to measure the retro-active inhibition exerted upon Section I. Note that entirely different and completely trained control groups would be required for measuring this inhibition on Section II and Section III, provided the single group failed to demonstrate its ability to run not only Section I, but Sections II and III in turn. Table XII, page 75, embodies the data. Inspection reveals practically absolute control of the successive units.[4] Hence, only one group was employed for the testing of all the sections. When the entire group required for the complete relearning of the four sections but an average of .6 trials, 3.9 seconds, and .65 errors per section (with all the relearning effort being directed to one section and herein expended mainly by a single blind rat), it is obvious that retro-active inhibition must be disregarded as an element of the great waste in learning the maze by the 'part' method.

(d) Loss due to contiguity of unit functioning.

It has been shown that learning a section does not interfere with the acquisition of a section on subsequent days (Transfer). Also, it is clear that the mastery of a new section does not interfere with the running of previously learned sections, provided that at least a day interval is allowed between tests. (*Retro-active inhibition*). Finally, all the motor units learned may function perfectly, provided a day or more elapses between the trial acts. (*Retro-active inhibition*). But it is pos-

[4] Attention is called to the fact that Section III alone presented difficulty. This difficulty is almost negligible, since 2.2 errors for a group is of course practically to be disregarded. It will furnish cold comfort to some of the present day animal psychologists to be told that the errors of Section III were made by two rats, the first, *completely blind,* whose relearning effort required seven trials, nine errors and fifty-three seconds, the other, three trials, two errors and fourteen seconds. These two rats alone determine the scores for Section III. If the blind rat were excluded, the averages for the group would approach zero.

sible that two acts might function successfully without any interference between them when there is interposed this considerable time interval, and yet that marked interference might occur if these different motor habits were forced to function immediately in succession, a condition that maintains in the connecting act of 'part' procedure. The difficulty so clearly demonstrated in the connecting act in 'part' learning may consist primarily in an interference resulting from this contiguity of function. The validity of this hypothesis had to be tested, not only in the case of motor acts learned in immediate succession, but for all possible combinations of the total collection of acts at the disposal of the subject.

A new group of rats (six in number), was taught the four maze sections. Retesting was made upon various sections as soon as Section IV was mastered. Here, differing from the group reported in the retro-active inhibition test, each rat was given but one run per section and then changed immediately to one not successively learned. This requires two distinct adjustments. All typical combinations were tested. These, for successive days, were I & III, II & IV, IV & I, III & I, IV & II. These tests of five days produce almost perfect results. In no case did the group average higher than 2/5 errors for the day. Most individuals of the group were able to adjust immediately to any such combinations and to accommodate to these changing requirements day after day. Finally, the daily task was increased by compelling double the general amount of work and this in the inverted order of learning, namely, successive runs in IV, III, II and I. This increasing demand in amount of work and complexity failed absolutely to create a breakdown in control. Three of the rats ran the entire four sections perfectly, each of the remaining three attained a single error for the entire four problems. The scantiness of errors renders untenable any opinion that the 'part' learner does not have control over the specific units he has mastered. This control exists irrespective of the order in which the units are required to function or of their functional contiguity.

(e) Loss due to unit incompatibility in a larger series.

It is logically possible that the various units present inhibitory tendencies one to the other in the act of connection and that these units have to be destroyed before the final act of union can be made. In other words, it needs to be shown whether any motor unit can function as a specific part in a bigger motor situation. Section (d) above merely showed that the units can function in temporal contiguity. It argued nothing regarding whether a definite part of a total act can function independent of that act. Obviously, if a group having mastery of the entire motor situation can run all parts of that situation as parts and various combinations of these parts, the questions of incompatibility of units and their inability to function as parts of a whole must be answered negatively.

The rat and human groups having been taught Maze A in the most advantageous fashion ('whole' method with returns prevented) were tested upon the various parts. (These so-called parts are the four *units* mastered in 'part' learning). By the removal and insertion of panels in the rat mazes and metal plugs for the humans, new connections were easily made possible, yet the character of the parts was unchanged. It was considered essential to try the rearranged parts in the forward learning order and the more crucial condition of inverted learning order. Hence, the subjects were tested in successive order upon I to III, II to IV, and IV to I Maze constructions. Between the completion of each such test, the subject was retrained upon the maze as a whole. This not only tested his ability to add to a modified act all the original parts but prepared him for an equitable attack upon each novel construction.

The behavior in the several changes was typical throughout for both rats and humans. A slowing up of speed at the new junction and an occasional retrace were followed by a headlong dash into the new section. No retracing occurred in the new section. An inspection of Tables XIII and XIV, p. 75, shows the amazing accuracy of both rats and humans in running the sections in variable order. This points to the fact that a motor unit may function as such, *provided it has been mastered as part of a whole.* It shows that no incompatibility as between

specific parts exists in the motor problem. Again, it enforced the conclusion reached in (d), that the sections have no inherent interference when functioning in immediate contiguity. Taken in conjunction with (d), it proved that the difficulty of putting the parts together is because the parts were learned as unit wholes.

From the above series of tests, certain definite conclusions may be stated, these having reference to the alleged causes of waste in 'part' learning. The conclusions apply for animals and humans.

(1) Learning one motor unit does not render the mastery of subsequent units more difficult. Transfer is strongly positive, thus pointing out a clear advantage of the 'part' method.

(2) Practically no disintegration of the motor habit occurs during the time between initial mastery and the final connecting act.

(3) No retro-active inhibition is exerted upon motor habits by the learning of subsequent ones.

(4) Different motor units may function as units in any order. Contiguity of unit functioning fails to disturb the motor habits.

(5) Parts of a motor act present no incompatibility to each other when they are learned as parts of a larger motor situation. They may function perfectly as parts, in any successive combination of parts, or in the entire motor series. Their capacity for part functioning is never lost.

The above generalizations emphasize the necessity of excluding as factors of waste in 'part' learning whatever refers to the mastery of the several units or the interrelationship between these units. By these elimination tests, the writer is led to conclude that waste in the maze problem occurs only in the act of connection and is here traceable almost entirely to the influence of place association. This hypothesis is discussed and tested at length in the following chapter.

CHAPTER V

Place Association and its Relation to Improvement of the 'Part' Method

The universal inferiority of the 'part' method has been demonstrated. Numerous proposed causes of waste in 'part' learning have been tested and rejected. Chapter IV brought out the fact that the writer relies mainly upon place association for an explanation of the poor results obtained by this method.

Place association refers to the definite location of an element of a problem in reference not only to the remaining details of that problem but to the entire environment. In the case of rote learning a certain syllable is learned in reference to its antecedent and consequent (immediate association) and to the remainder of the terms (mediate association). It is hereby assigned a definite position in the word series. This places it in a conceptual scheme. It is located at a definite number of syllable intervals from both the introductory term and the terminal one of the list. It is reached after the same time expenditure in each trial and is followed by a constant time span for the completion of the presentation. Both spatial and temporal factors are concerned in establishing the positional relationships.

In motor learning of the maze type, the establishment of place associations represents a large part of the learning. These associations are no doubt very complex. Certain ones may be indicated. (a) Time. The learner comes to relate a certain time span to a certain change of activity. Specifically, a short time run for the rat means a cessation of the running activity and the substitution for this of feeding. Also, it is logical to suppose that each critical turn or element of the maze pathway is located (though not in a conceptual sense) in the entire time span just as definitely as a term is located in a series of nonsense syllables. (b) Distance. The learner

is taught to run a certain distance and secure a desired change of activity. In the case of 'part' learning, each section requires that the same distance be traversed. Consequently, the learner attacks his daily problem with the expectation of having it solved when certain clearly defined time and distance demands have been satisfied. (c) Details of the maze pathways. Each turn, cul de sac and section of the true pathway become positionally established. A given corner may be located in reference to many factors, e.g., the opening into the food-box, the starting place, the next cul de sac, the electric lights, the position of the experimenter, etc. Each aspect of the course is no doubt associated with and located in reference to all the details of the course and to the entire objective environment as well.

The above suggestions may not be exhaustive. It seems to the writer, however, that they state the main types of place associations that are set up in learning the maze problem. Also, it seems logical to assign the difficulty of the act of connection in 'part' learning to the break up of these specific positional factors. If they are causative of the waste in 'part' learning, the behavior of the learners should reveal it. Again, the evidence drawn from the previous experiments must support the hypothesis. Finally, the factors of place association must be so experimentally treated as to show the exact way they are operating to condition waste. Such a treatment of these factors would produce better learning results than were secured by the pure 'part' method provided the factors are eliminated or negated to some degree. This would demand the devising of improved methods of 'part' learning. The task of the chapter is given to the three necessary lines of procedure stated above.

(a) Behavior.

The behavior of both rats and humans in the act of connecting the successive sections was described in Chapter II. It may be characterized as passing through ten distinct stages. (1) Free and unchecked. When started at the remotely learned Section I, the subject "got his bearings" and proceeded rapidly and accurately. (2) Break down in control. This occurred

at the closed exit to Section I. It is characterized by a cessation of forward directed activity. (3) Testing of old habits. The subject might retrace or dash into Section II. He had learned the meaning of the retracing habit when the pathway was blocked (e.g., in a cul de sac) and also the going ahead habit. (4) Failure of habitual adjustment. Retracing brings failure. Arrival at the exit of Section II (generally the exception for the opening trial at connecting) brings like results. The run through Section II was irregular, wavering, and generally given up, the subject returning to Exit I and then into Section I. This stage is characterized by the development of a strong emotional factor, roughly to be designated as confusion or lack of confidence. (5) Random activity. Here complete inability to handle the situation is manifest. Aimless darting into alleys, incessant complete and partial returns, complete cessation of activity followed by rapid attacks are evident. (6) Directed activity. The subject settles down to the problem, relying not on specific control of units but upon his general maze knowledge. This stage is one characterized by persistency. (7) Accidental success. No less than in the first act of learning, this trial and error process brings the desired result. Judging by the behavior upon the last few sectional passageways, the subject had little, if any, knowledge that he was approaching the desired goal. Following the first successful trial, the subsequent trials suffice for the (8) fixation of the useful movements, (9) elimination of the useless, and the (10) final complete organization of the sensori-motor connection.

The above analysis of the behavior in the act of connection shows clearly the complete breakdown of control. Short time and distance relationships absolutely fail to bring the changed activity previously secured. A specific maze corner ceases to mean a turn "to be followed by food getting." It is now a turn that leads to a new situation, calling for far more time expenditure, more distance to be traversed, etc. The several closed exits represent the critical points where old habits fail. Here the subjects halts, explores the situation, and shows in

every possible way that his control over the motor situation has broken down.

(b) Comparison with previous tests.

It was demonstrated in Chapter III that each unit of the maze could function perfectly as an element. This ability was shown to maintain irrespective of the order of functioning of the several units. This fact argues that positional factors are never disturbed so long as the motor habits are allowed to function as units, but that the connection of these unit habits into a series immediately brings disturbance. It was also demonstrated that elements learned as parts of a whole could be put together in any fashion without difficulty. Such operations did not call for an extension and enlargement of short temporal and spatial relationships. Rather, they represent cases where the subject reaches his goal with less time consumption and less distance traversed than is customary. This difference emphasizes the writer's general contention. It seems certain from these facts that the place associations set up when the parts are mastered have such great strength that they render the act of serial connection extremely difficult.

(c) Experimentation directed toward the elimination of the positional factors.

It is obviously impossible to devise modified methods of 'part' learning where some positional factors (short time acts, short distance traversed, etc.) are not established. No test can be devised to eliminate all these factors at once. It is necessary to eliminate these progressively and in the most advantageous fashion. The tests to be described have value to the extent in which they eliminate or negate to a degree some one or some group of the positional factors. The nature of each new test and the learning will be treated comparatively. A presentation of the learning scores and an appropriate evaluation of each method are reserved until this preliminary survey of the methods and the learning behavior has been made. (See pages 39 sq.)

(1) 'Direct Repetitive'.

Rat groups and humans were trained upon Section I until

mastery was accomplished. At this stage, the individual subject was required to run through the mastered Section I into Section II. This change in the maze pathway was rendered possible by the removal of the dividing panel and the closing of Exit I. When mastery of the I-II course was completed, III was added to the accumulating series, finally IV. In each modification, then, the subject was required to repeat the familiar area and to enter the strange. A review of the mastered sections was hereby given in each trial. Furthermore, the place associations set up during the mastery of Section I were reconstructed as soon as mastery was attained. The problem was made to expand. 'Part' learning called for an isolated attack upon Section II, this and subsequent sections serving very largely to make more deeply seated the short time and short distance factors of each maze unit. The identity in length of the four units argues for this. But by this 'direct repetitive' method, however, the positional factors are no sooner set up than they are made to relate to a larger, more complex situation.

The behavior of these groups was characteristic. Upon finding Exit I blocked, the rat usually proceeded very cautiously into Section II, generally making numerous partial returns to Exit I. Seldom was retracing continued through Section I. The human behaved the same way, but the retracing was probably less marked. Both for rats and humans, there was little or no hesitation after the second trial upon the arrival at Exit I. The same is true for retracing. The entire efforts of the learners seemed directed to the mastery of the final, unfamiliar unit. The speed of approach to this attack was generated by the running of Section I. It shows the operation of a factor roughly to be considered the *influence of the known.* It bespeaks for the method a favorable "warming-up" period. (See Tables XVII & XVIII).

(2) 'Reversed Repetitive'.

Rat and human groups were trained upon Section IV until mastery was attained. As soon as the individual mastered this final unit, he was required to attack the third, working through this into the previously learned IV. In turn, he added on as the first part of the accumulating motor series Sections II and I.

In each modification, then, the subject attacked the novel and ended each trial by traversing the familiar. The problem may be stated as testing the *influence of the unknown.* This method of learning is, therefore, the reverse of the 'direct repetitive' described above. It consists essentially in learning the maze backwards, as opposed to the forward aspect of the previous method. Each trial calls not only for the partial mastery of a new section (the first part of each run) but for the final review of the previously mastered units as well. Both these 'repetitive' methods differ from the 'part' method in the fact that the various sections are not mastered separately. The value of these repetitive methods seems obvious. Place factors never become strongly established. This is doubly clear in this last mentioned method ('reversed repetitive'). Herein the subject has never learned habits of stopping, except at one particular place, *i.e.,* the open door of the food-box. The following paragraph on the behavior in this learning method shows why this is true.

The behavior of these groups differs from that of the 'direct repetitive' type. Usually, after the learning of general maze habits in Section IV, the new problem was attacked eagerly. When the entrance from III into IV was reached (a place where the subject had never learned to stop) recognition with the rats was extremely obvious. Speed was quickened and Section IV run with precision. This general behavior was manifest for all the successive modifications. There was never any stopping at a closed door, for the subject had never made any associations of food getting, changing of running activity, etc., with this. Rather, the closed door meant the entrance to a familiar maze section, one that called for quickened speed and the satisfying of the desired activity at the same identical terminal point. But the recognition cue in the case of the humans failed to function as definitely.[1] (See Tables XVII and XVIII.) Conse-

[1] Maze studies of Carr and Watson (31 and 20) seem to argue for the strength of the kinaesthetic cue as the recognitive agency in the maze situation. The evidence of the present research assigns most of the recognitive capacity to vision. Failure of recognition with the humans may be due to lack of vision. Of course, the question whether the human had reduced the control to the kinaesthetic level is involved.

quently this method fails to score very high with the humans.

(3) 'Progressive Part'.

Rat and human groups were trained in Section I, and then taught Section II as a new problem. Connection of Sections I and II was then required, this being followed by a mastery of III and its addition to the I-II course. A final tuition on IV and its addition to the I-III series completed the experiment. This type of learning resembles the part method in that the sections are learned as definite units. But these parts are made to function as soon as the first two are learned in a bigger, more complex motor situation. The strength of place associations is not continually on the increase, as is the case when four equal units are mastered in pure 'part' learning. Rather, single groups of these positional factors are progressively eliminated. Consequently, the method may be termed 'progressive part'. *The influence of successively learned additions* is hereby measured. Herein the difference between this 'progressive part' and the 'repetitive part' methods is clearly seen. The latter call for a review of the mastered areas in conjunction with the learning of the new. Two types of activity are present. The former method demands either the exploring activity or that different type required for the connection of mastered sections. These cases are quite different. The significance of this fact will be commented upon later in the chapter. Also, these methods relate differently to the positional factors. The 'direct repetitive' method demands a disregard of the place associations set up in reference to the exit of the first short maze section and an entry into an unknown area. The 'reversed repetitive' method calls for an initial attack upon a novel situation, this being terminated by a recognition of and a hurried traversal through the known areas. Little unseating of place associations is demanded. The 'progressive part' method demands a disregard of the place associations as in the 'direct repetitive' case. However, such disregard is far easier because the area to be entered is entirely familiar and is that which has just been mastered. The difference of these three cases is highly significant.

The behavior of rats and humans was strictly parallel. Sections I and II were mastered in regular fashion. The connection of the two presented little difficulty, not nearly the degree manifested by the original 'Part' Group. It was shown in Chapter II that few of these 'part' learners progressed as far as Exit II without errors. (Note that such a group has four units mastered rather than two). In fact, most of the subjects, both rats and humans, effected the connection without errors of any type. The subsequent learning of III and IV and their successive additions follow the description of easy learning just stated for I and II. Certain rats and humans effected the entire series of combining acts without error. The behavior of each subject clearly shows that the elimination of the place associations is rendered very easy by this 'progressive part' method. (See Tables XVII and XVIII.)

(4) 'Elaborative Part'.

This experiment was not planned as an improvement upon the part method but represents one of the happy accidents that occasionally turn up in a research largely directed into the dark. The work was restricted to rats. It is included here for suggestive and not comparative purposes. Also, it throws some light upon the results of the 'modified part' methods just described. The work is here discussed in detail, as it will not be referred to in the subsequent discussion of results (pp. 39 sq.).

The test group of rats was the same as that reported in Chapter IV, Section d. This group had been trained as for 'part' learning except that no act of connection had been allowed. Hence, the rats had command of four distinct maze habits. They were tested upon succeeding days for their control of the units, taking in succession various changing pairs. On the sixth day, the rats were required to run the entire collection of units but in the order directly opposed to that finally to be desired.[2] The extreme skill in doing this has been com-

[2] This complex reviewing of the units in no sense altered the place-association factors set up for each motor unit. Rather, it required the rat to adapt quickly to a change in sensory conditions. It forced him to utilize successively all the motor habits at his disposal. It prepared him to attack any maze situation without delay. In these respects the 'elaborative

mented upon. (See page 26.) Immediately at the close of this IV, III, II, I testing, the rat was placed in Section I and given opportunity of connecting all the parts. The data are recorded in Table XIX. These are itemized for the specific runs given and are complete up to trial four. By the end of this trial the *entire* group had mastered the entire maze. (Trials 2-4 were given the day following trial 1).

Significant comment must be made regarding the connecting behavior. Two of the rats ran perfectly; three entered one blind alley; but continued the journey; one entered one blind alley and returned fifteen sections to the doorway, this being followed by a perfect run. The time of this initial trial was remarkably low (41 seconds). The ability to connect the sections certainly seemed amazing in view of the behavior manifested by the 'part' group reported in Chapter II. The method closely duplicates the original 'part' method. The sole differences are that the unit sections were run in immediate succession as units just before the connecting act, and that there was a review of the parts during the several days just preceding the connecting trial. This is responsible for the great difference in results.

Certain causative factors are perhaps statable.

(a) It may be that there is a slight loss in the ability to control the specific motor habits, this being traceable to the disintegration through time and to retroactive inhibition. But such a loss certainly proves non-effective in causing disturbance when the sections are run simply as units. The general adaptive powers of the organism may prove too weak, however, when the four motor habits are required to function together, and in conjunction with disturbances of place associations. Hence, the 'elaborative part' procedure has its value not in running two or more sections on the same day, but in practicing the various sections separately and hereby eliminating loss. This is a 'practice', a 'warming up', or a 'refreshing' theory.

part' method differs from the pure 'part' method. The two methods do not differ in so far as the former has specifically eliminated certain of the positional factors before the final act of connection.

(b) The difference between the 'part' and 'elaborative part' scores may be due to place association. In 'part' learning, the rat learns a series of acts,—runs one section; eats food; runs the same section; eats food; removal to cage. This is a unitary series and is completed each day. In the 'elaborative' method, the final procedure is different. Here the series of acts learned is illustrated by the following procedure:—runs Section I; eats food; runs Section II; eats food; etc.; removal to cage. This is wholly different from the series established while mastery of the separate units was being attained. It breaks up this earlier series of acts. This break-up is not so great but that the rat can adapt to it. Yet it is sufficient to make the transfer to the connecting situation (I-IV) far easier. In other words, it might be possible to proceed from four unit sections to linking these without a manifest disturbance, provided such was accomplished by gradual steps.

(c) In linking four units together, positive association must be established. Some connection may be established while the units are being learned, for all the units are parts of a common situation of food, location environment, experience, etc. The act of linking requires a closer association and this is accomplished by contiguity,—functioning in immediate succession. The connecting act of 'part' learning serves this need. The 'elaborative part' method aids the establishment of the final close association by bringing the units together in time and in succession for the first time, and yet in such a way that the distractions which cause errors are not present. The units are first brought together two at a time instead of four, and this proceeding by easy stages is advantageous.

(d) On first thought, the difference in scores for the 'part' and 'elaborative part' groups may be largely due to group differences. This hypothesis is worthless, as the behavior and records of the two groups clearly show. (See pages 11 and 37.)

(e) Some possibility unnoticed by the writer may be explanatory for the difference of results. The above points are merely suggestive and it may be that they are not exhaustive for the situation.

Leaving the speculative treatment of the 'elaborative part' method, there are certain general conclusions that may be drawn from an inspection of Tables XVII and XVIII, page 76, regarding the 'modified part' methods.

(1) For the rats.

The three modifications of the 'part' method all prove superior to the 'pure part' method and to the 'whole', irrespective of the favorable blockage of returns. Superiority is demonstrated by all the criteria of measurement (time, trials, and total errors), except for the total error criterion in the case of the 'direct repetitive' method. Here the total errors exceed the number in the 'whole' method with returns prevented. This difference is no doubt due directly to an allowable retracing (Type C error) in the case of the former method. This exception, however, disappears by making a comparison with the forward cul de sac (Type A) error. The 'part' method and the 'whole' method with returns allowed prove inefficient as learning methods.

(2) For the humans.

One of the modified part methods ('progressive part') proves superior by all criteria of measurement to the 'whole' and 'part' modes of maze learning. The 'direct repetitive' method proves superior by all measuring criteria to the 'whole' method with returns unprevented. However, this 'direct repetitive' method requires more time and develops more errors than the 'whole' method with returns prevented. This difference again disappears by making a trial comparison with Type A errors. Time was lost and errors accumulated by the possibility of returns, as the data show the errors of the B and C type are higher than in the 'whole prevented' method. The 'reversed repetitive' method falls to fifth place and its location is likewise definite for all criteria of measurement. The enormous number of retrace errors (Type C) is due no doubt to failure in recognizing the mastered section upon reaching it. Here the behavior and records differ markedly from the rats and raise the question of kinaesthesis functioning as a recognitive means (see note, p. 34). The pure 'part' method is last in the list, irrespective of measuring criteria.

(3) Rats and humans.

The 'progressive part' method proves universally superior for all types of learning methods. The 'reversed repetitive' is highly favorable with the rat but less so with the human, this difference being statable in terms of recognitive ability. The 'direct repetitive' method is for both groups more favorable than 'part' learning and, in general, than for 'whole' learning (See exception in 2 above). It thus shares favorable universality with the 'progressive part' method. The 'whole' method when returns are prevented is universally superior to the case where returns are allowed in so far as regards time and errors but not the number of trials; in comparison with modified 'part' methods either 'whole' method is inferior. The 'reversed repetitive' method fails to prove efficient with the human, due no doubt to the failure in recognition of the previously mastered sections. The pure 'part' method is the most inefficient method used, waiving a single exception with the rats, this poor result being assignable to the unlimited possibilities of retracing. It is interesting to note that the number of trials, number of seconds, and number of total errors for mastery by a certain method are in absolute terms, universally less for the human than for the rat.

(4) Correlations.

Table XX shows roughly the (a) correlation between the different measuring criteria for all types of motor problems devised. For both rats and humans this correlation is high. It argues that rats or humans manifest high regularity in time and energy expenditure for various motor methods. It shows that there is no royal road to mastery for the human not open to the rat. Also, it shows that correlation is very strong between number of trials and Type A errors, but that this weakens in comparing Type A errors with total time or total errors.

(b) The cross comparison for rats and humans shows that there is good correlation in respect to the number of trials required by the various learning methods for mastery. There is much less correlation when the measurement is in terms of time expenditure or the accumulation of errors. The error measure-

ments show that the highest correlation exists between the Type A errors (forward directed cul de sacs).

The above experiments have been directed toward the verification of the place association hypothesis proposed in the earlier sections of the chapter. They have shown that the positional factors may be so progressively eliminated in various forms of 'part' learning as to render these forms (*i.e.,* modified 'part' methods) much more efficient than the original 'part' or 'whole' methods. At the risk of repetition, it seems advisable to restate certain favorable aspects of these efficient modified 'part' methods.

(a) Progressive elimination of the emotional factor.

Remembering the indecision, random activity, full stopping, etc., of the 'part' learners when in the act of connection, its absence in modified 'part' behavior is significant. With the 'progressive part' method, the learning of Part I and II arouses a complex emotional state which must perforce be overcome while these parts are being mastered. The connection of these fails to re-arouse indecision, fear (with the rats, especially), etc., for the entire course is a known safe one, presenting but a single novel feature, *i.e.,* the connecting unit. This seems to be attacked without bringing any strong emotional accessories. Part III is again a new but relatively less emotion-provoking situation. Its addition to the known course follows the subsidence of the emotional complex. Again, the task presents but a single new feature, comparable to the like feature previously met. The course to be traversed is a longer, but still a safe course. Part IV is learned without arousing to a significant degree any emotional tone whatever and the various motor acts have been so progressively interrelated as to call forth little if any of this emotional disturbance during the final combining stage. In general, the only occasions for the arousal of the complex are during the short task of unit learning (where the state must be eliminated before final mastery) and in the single act of connection (the three successive acts becoming successively easier). With the pure 'part' method, the mastery of each part has called for the arousal and subsiding of fear, hesitation, etc. The task of connection involves, therefore, a re-arousal of the injurious

emotional tone at the three critical connecting points. And the strength of this is cumulative. The connection of the first two maze elements arouses it very little, but the subsequent addition of the remaining section brings very marked disturbance. Neither rat nor human succeeds in avoiding this. The 'direct repetitive' and 'reversed repetitive' methods demand a rhythmic arousal and subsidence of the emotional factor, this having a degree of strength far greater than with the 'progressive' method. It is simultaneously involved in mastering the added unfamiliar section (whether approached from the familiar or leading to it) and in affecting the junction of the two. The emotional complexity logically results in poorer learning scores, an hypothesis admirably supported by the data. However, this becomes relatively less operative for the final additions. In all cases, this complexity is much less than is produced by the act of connection in the 'part' method. It is clear, therefore, that the emotional element, though always present, can be distributed to one or several definite maze points and progressively eliminated. This is impossible with 'part' learning and likewise with 'whole' learning, where numerous tricky, blocking situations are met with for many successive trials.

(b) Progressive elimination of the positional factors.

(1) Temporal. Partially causative of the emotional disturbance are, of course, the time relationships. In 'part' learning, each run has been shown to correlate with a brief time span, this ending with changed sensory conditions of desirability. In learning two short units, the brevity of the temporal series is not so firmly a part of the subjects reacting system as when four had been mastered. Hence, the tendency to stop after running a single unit is more easily modified. Also, the overcoming of this stopping tendency is always followed by success and with utilizing but one, short-time activity. As the accumulation and connection of parts proceed, the successive demands never call for but one such short-time addition. The temporal series progressively increases but always by a definite, short-time, success-bringing act. With the pure 'part' method, the subject knows little beyond a relatively high number of deeply intrenched short-

time acts. The break-up of one of these temporal relationships is relatively easy (though harder than if only two had been established), but this is unattended with success. Neither are the immediately subsequent ones success giving, for only the last and most recently mastered time unit can so function. The demands upon the time relationships are invariably too great. With the other forms of modified 'part' methods, no short-time factor ever becomes strongly seated. As soon as one is set up, it is immediately modified by an addition of like extent. The erection of the temporal side of the final maze situation is progressive throughout. This progression is by stated, constant, temporal units and differs herein from the 'whole' methods. In the latter, theoretically, the final time series is under construction from the beginning of the tuition period. By preventing returns (by cutting the course into four sections), not one but four rival time units of the final total are being constructed, and not only as units but as parts of an indefinite whole. With returns allowed, probably many more such units are set up; having to do with all areas of difficulty of the course. In neither such case, therefore, is the erection of the time series progressively or chronologically made. Temporal habits of stopping are not deeply engendered, but the final series is slow in being attained. It is evident, therefore, that the values of the modified 'part' methods rest in large measure upon (a) progressive elimination of the positional factor of time and the fact that the (b) final time series is successively extended by short regular additions as opposed to an internal adjustment of a constant and highly complex whole.

(2) Spatial.

Spatial factors function in like manner. The position of each section and turn (no doubt especially so for final turns) is mediately associated with the spatial terminus of the run. The last turn means open doorway, food getting, and a complete change in sensory factors. With 'progressive part' learning, relatively few of these are set up by mastering Sections I and II. Those that are indicative of arrival at Exit I do have to be uprooted in the first connecting act. Their poverty of

number is in their favor. Once eliminated they function in the larger series. Each addition to the expanding series calls for a readjustment but this is always directed towards a constant goal, constant results, and over familiar territory. Many of the adjustments are primarily with an unconnected but familiar unit, not internal to the great mass already satisfactorily arranged and now functioning as a unit group. This spatial demand is, therefore, progressively met. With connection of many parts being required, the demands of spatial readjustment obviously are multiplied to a high degree. Suppose each section requires for mastery the establishment of at least two positional factors. Using letter denominations, in Section I are established factors A and B. But logically and empirically these are to have unit functioning, *i.e.,* as AB. Mastery of Section II required the establishment of C and D, but these are reduced to the single CD unity. The immediate connection of the dual AB and CD groups again involves but two adjustments. Section III requires two establishments for E and F, but their reduction to unit functioning requires only two new establishments with the ABCD unity. In short, for the complete mastery of these hypothetical situations, there is demanded the establishment of only fourteen such relational factors. The small number is traceable to the demonstrated capacity of a complex, automatized group to function as a unit in needed adjustments to an external situation. No internal readjustments of great degree seems rationally or empirically needed.

With the 'direct repetitive' method the first section reduces the two adjustments, A and B, to a unit. When this unit adjusts to the new, now-to-be-mastered, C-D situation, the permutations are between three terms, namely AB, C and D. Consequently six adjustments are required to establish the ABCD unit. With the addition of E-F and again of G-H, the adjusting demands remain six for each such addition. This method requires, therefore, a total of seventy such spatial positional attainments. The same number holds for the 'reversed repetitive' method. In both cases, the demands of read-

justment of the spatial factors are successively met. The demand is initially never high and it becomes increasingly easy. Yet these 'repetitive' methods always require a greater number of adjustments than the 'progressive part' methods and the character of these adjustments is more complex. This increase in complexity of character depends mainly upon the demand for a triple accommodation (*e.g.,* AB with C and D) as opposed to a dual demand (*e.g.,* AB with CD).

The spatial complexity in the remaining methods is obvious. In 'part' learning, the four units require a minimum of eight positional establishments. When each pair is reduced to a unit and thrown into the connection series, the four units functioning without errors require twelve combinations, bringing a total to equal either 'repetitive' method and to exceed the 'progressive part' by 43%. But even this numerical equality is deceiving. The permutations are not in reference to single successive functional units but to remotely successive as well, and both forward and backward in direction. Rationally, then, the task is hard. Empirically, the ability of units to function as a whole is destroyed. A veritable dissociation of component maze elements takes place. The subject, having had the mastered groups broken up, begins the new and highly complex task of erecting a bigger, positional series out of the wreckage left him. The maze records show that he does this little or no better than if he had had no earlier sectional training. (In the case of the rat, the group seems almost the worse for the training. Tables I and VI reveal that the connecting act required almost the time of, and accumulated more errors than, 'whole-prevented' learning. Tables III and IV show that conditions were even more disturbed for the human, even in the 'whole-allowed' case). In 'whole' learning, with all eight A-H establishments simultaneously in demand, fifty-six combinations are needed. In the 'whole-prevented' method, the same number is required, but the arbitrary cutting of the maze into four sections may tend to reduce sectional pairs to relatively early and complete unity. This has its reward in the saving of errors and time. This possible economy is spent with 'whole-allowed'

learning in setting up the numerous far-distant and backward directed associations.

It appears, therefore, that values of modified 'part' methods in comparison with pure 'part' and 'whole' methods are statable mainly in the progressive and distributive handling they furnish to the positional factors, whether these are considered as emotional or in more objective forms of time and space. Such conclusions have apparent justification in logical, mathematical, and empirical sources. They argue that the relative advantages of the various 'part' methods must be due mainly to the degree in which place associations are obviated.

The results of the employment of modified 'part' methods for the elimination of place associations may be summarized as follows:

(1) The behavior in the act of connection, the conclusions drawn from previous tests, and the data secured by utilizing modified 'part' methods show that place associations render the act of connection in 'part' learning extremely difficult.

(2) These injurious place associations are statable in both the temporal and spatial series.

(3) Modified 'part' methods are originated which eliminate or negate to a degree some of the harmful place associations.

(4) These modified 'part' methods prove far more efficient than either the pure 'part' or 'whole' methods. This is true for rats and humans.

(5) These methods have been named the 'progressive part', 'elaborative part', 'direct repetitive', and 'reversed repetitive'. The relative values of these vary for rats and humans. Differences are statable in terms of the recognitive capacity.

(6) The value of these methods consists mainly in the progressive and distributive handling they furnish to the positional factors.

(7) There is no royal road to mastery for the human not open to the rat. Both rats and humans manifest high regularity in time and energy expenditure for various motor methods.

But the question immediately emerges regarding the superiority of these 'modified part' methods over the universally

efficient 'whole' method. The partial elimination of the positional factors set up in the mastery of the separate maze areas not only improved the scores secured in 'part' learning, but produced results far superior to those of 'whole' method learning. If these positional factors had been entirely eliminated, it looks as though the results should merely have equalled those secured by the 'whole' method procedure. But the universal superiority of certain of the 'modified part' methods argues at once that there are certain inherent values to 'part' procedure. A further analysis of these part learning methods must be made, with a view to ascertaining their inherent advantages. Such an analysis is attempted in the following chapter.

CHAPTER VI

Elements of Advantage in 'Part' Learning

The results of the preceding chapter are highly significant. It has been shown that 'modified part' methods can be devised which prove far superior to the pure 'part' method of learning. The improvements produced by these new methods have been shown to depend partly upon the progressive and distributive handling they furnish to the positional factors of the temporal and spatial series. But a result of far greater significance has been obtained. These 'modified part' methods prove superior to the 'whole' method, even when this latter method is operating under the favorable condtion of blocked returns. From a logical viewpoint, this result seems improbable. A method which obviates some of the weaknesses of the 'part' method (*e.g.*, place associations) should produce scores that approach the results of 'whole' method learning as a limit. If place associations were the only differential aspects between 'part' and 'whole' method learning, the 'modified part' methods could never excel the 'whole' method. Indeed, the impossibility of devising any modifications of the 'part' method that do more than partially obviate some of the injurious place associations is frankly acknowledged by the writer. It is clear that there are factors operating in 'part' procedure, which are producing the remarkable scores.

The fact that learning scores are inferior under the 'part' method must not blind the reader to the significance of its advantages. These favorable factors may have been operating and yet been apparently submerged by the demonstrated weaknesses of the method. Again, the conclusions long ago reached regarding the learning of verbal material must not blind the reader. The investigators in this field showed the inferiority of pure 'part' learning, as has the present research for the motor field. But none of these former experimenters have

attempted to modify the connecting act. No one has been in a position, therefore, where he was forced to recognize the fundamental advantages of any 'part' method. Such a recognition logically depends upon empirical findings similar to those of the writer, namely, that modifications of the 'part' method not only equal the 'whole' method in efficiency but prove far superior to it. The writer is not arguing against the present conclusions regarding verbal material. He is merely pointing out that there is an angle of the problem not yet faced by the investigators, and showing the conditions in motor learning that make such an issue seem vital.

There seem to be certain obvious advanatges to any 'part' procedure in maze learning. These operate to produce learning scores superior to 'whole' method results.

(a) Transfer.

The present work of the writer has presented definite data regarding the maintenance of transfer. (Chapter IV, pp. 22-23.) The formula for its estimation takes account of the trials, time and total errors and gives a mathematical result that is easily interpreted. The results tend to show that transfer is progressively increasing through the learning of four successive maze habits. (See previous conclusions, p. 22.) If the formulaic estimations are made for the successive stages of the 'modified part' methods, the same conditions of positive transfer are again seen to operate. The conclusion is that subsequent maze habits are mastered far easier than the earlier ones.

Two questions immediately emerge. (1) What are the transfer items that render successive maze habits more easily set up? (2) Do not these operate when the maze is being learned as a whole? Does the learner fail to master the final sections of the maze (specifically, the final three quarterly divisions) with a progressively decreasing energy expenditure? The writer can do little more than speculate regarding the answer.

(1) Transfer items in learning successive maze habits.

General. By general transfer is meant that there are certain habits or attitudes that can function unimpaired in any new

maze situation. These general items refer in no sense to the details of the new maze pattern, but solely to the general character of the problem. Chief of these is probably the *general maze habit.* Several definite elements are involved. (x) Retracing. The dominance of the familiar has often been commented upon. The return pathway is known to be safe. The rat seems natively inclined to ɪeturn to the closed entrance. Final maze mastery means the complete elimination of this retracing. Learning any maze—long or short—actually inhibits the retracing tendency in subsequent maze learning. (y) Knowledge of the nature of errors. A single maze mastered suffices to teach the learner the concrete meaning of the blind alley. A cul de sac ceases to be a detail that must be cautiously explored. It comes to mean a condition that must be left as soon as possible. (z) Sense of direction. Some learners have almost a "going ahead" instinct. Others become hopelessly confused when leaving a blind alley and learn only through repeated trials to make the turn that leads away from the closed entrance. In subsequent mazes, the truly sophisticated learner will enter the cul de sac, but will proceed along the forward pathway when he returns to the true course. These three elements are fundamental in the development of a general maze habit.

A second item of general transfer is *consciousness of power.* A maze learner spends many minutes in apparently aimless wandering. Hesitation, ceasing to explore the blinds, pausing to wash and rest, etc,, are indicative of the rat's indecision and lack of confidence. Even the human will argue his inability of getting through his first long maze. Nor does this lack of confidence become eliminated after the first successful trial. For many days the task is an arduous one and is approached with hesitation. With subsequent mazes, however, the consciousness of power is clearly seen. No 'warming-up' period is needed. There is no delay at the entrance. Work has come to mean invariable accomplishment and reward. The entire attack upon the new problem is aggressive. The learner has learned to do by previous doing.

Clearly associated with the above is a third general item, namely,—*proper emotional attitude.* It has been shown that a harmful emotional complex arises when the learner is first introduced to the maze situation and again when he is required to connect small maze units. It has been shown that final success cannot be attained until this attitude—a mixture of fear, indecision, curiosity, and perhaps anger—has been eliminated. In its place comes an attitude strongly conducive to success. Confidence, elation and hope may be descriptive of this. Irrespective of anthropomorphic criticisms, the writer is content to believe that the maze learner—animal as well as human—does attack the second maze problem with an entirely different and far more beneficial attitude from that which maintained throughout almost the entire first learning period.

Specific. By specific transfer is meant a certain definite maze habit that can function partially or unimpaired in a new maze situation. The writer is referring directly to the details of the maze patterns. Certain ones may be commented upon. If the first maze has taught the learner that a long run is to be followed by a turn to the right rather than to the left; by a turn of 180 degrees rather than 90; by a sharp, cautious turn of 180 degrees rather than a wide, safe turn of the same type, in so far as the second maze possesses like elements, specific transfer will tend to operate. A concrete example of this is found in Maze A used throughout the experiment. Cul de sacs numbered 3, 6, 9, 12 were constant in location for the four distinct maze units; were all approached by making a turn to the left; were each met with after the same time and distance factors had functioned; were the third and final cul de sacs for each motor unit. (See Figure I.) When the maze was learned as a whole, each of these errors was frequently made. Nos. 6, 9, and 12 (the final error) were especially numerous. In learning the four maze sections separately and successively (as did the 'part' learners), nos. 6 and 9 were rarely entered and no. 12 practically not at all. The maze had been designed with a view of establishing a partial identity of detail for the four sections, so that this element of specific

transfer might be partially tested. The present evidence, though limited, seems to argue for the transfer of this specific motor item.

General elements of transfer probably do not operate at their full value until the third or fourth maze is being mastered. It seems to the writer that they should progressively increase in strength to a maximum and thereafter remain constant. Specific elements of transfer probably operate differently. With an increase in number, these specific elements probably tend to generate an inhibition when later motor units are being mastered. No rule can be stated, but it seems to the writer that specific transfer should increase to a maximum (probably during the learning of three or four simple maze habits) and thereafter should operate with a progressively decreasing valency, even to a negative or harmful level. Controlled laboratory testing may refute these mere theories, and also the rough analysis given above of the transfer qualities. Certainly nothing beyond mere speculation is here proposed. The hope of the writer is that some experimenter will set to work to isolate the transfer qualities, both general and specific.

The above sections have been concerned with the principles of transfer that operate in rendering the second or subsequent maze habits more easily set up. Now, in that both 'part' and 'modified part' methods of learning call for the mastery of new problems after one or more related ones have been learned, it follows that the transfer factors can operate to their fullest strength when learning is by some part method. Transfer is fully utilized when the maze problem is broken up into unit sections. This is a great element of strength in any part procedure. But does this argue that these same transfer effects fail to operate when the maze is learned in toto? If not, a transfer hypothesis will fail to explain why 'modified part' methods produce better results than the original 'whole' procedure.

(2) Transfer in 'whole' method learning.

General. Adopting the analysis given above as truly descriptive of transfer qualities operating in 'part' learning, it is nec-

essary to see if these operate when a large maze is being mastered as a whole. First for consideration is the *general maze habit.* (a) Retracing. This is one marked characteristic of 'whole' method learning. By many partial returns and many frequent ones for the entire length of the return pathway, the subject finally learns that the retracing must be discontinued. Yet this retracing may continue until the last four successful runs. Chapter III brought out the fact that this retracing is probably useless and even harmful. The 'part' learners are forced to inhibit retracing when their problem is simple and when the retracing cannot take much time and accumulate many errors. They master this aspect of the general maze concept under simple conditions, the 'whole' learners under complex ones. Also, a knowledge of the uselessness of retracing gained through the earlier section of the entire maze fails to prevent retracing in the final maze areas. With Maze A, the greatest amount of retracing (after the earlier trials) was from the terminal point of the third division (the end of Section III) and error no. 10 in Section IV. With Maze B, the greatest tendency was to enter the final cul de sac and to retrace therefrom. It appears conclusive that 'whole' method learning fails to make full use of the general non-retracing concept in the final areas of the maze. 'Part' method learning is exactly opposed to this condition, for the final motor units are learned almost without retrace errors. (Tables I and III.) (b) knowledge of the nature of errors. The earlier sections of the total maze suffice to develop this concept. Were it not for the emotional complications, the latter sections of the maze could be run with increasing skill, just as if the areas were being mastered as successive parts. (c) Sense of direction. Here, again, there is no logical reason why the subject should not have learned in the first part of the maze which of the two possible turns from the cul de sac meant a return direction. Fear, hesitation, etc., are leading the runner to take the return pathway, however, so that the transfer values are not allowed to operate.

In the second place, the *consciousness of power* is almost

ineffectual. For many succeeding runs both the rat and the human proceed cautiously. Aggressive attacks come only with experience. And until the maze is almost mastered, a high initial speed will die down long before the final areas of the course have been reached. In 'part' learning, these final areas are mastered with great speed, but here the 'whole' method learners are the most tardy. Many acts of short duration, followed by desirable changes in activity, develop this needed consciousness of power. An act of long duration, carried out through many difficulties, develops this feeling after the time for its greatest utility as a learning tool has passed.

Regarding a *proper emotional tone,* it has been shown earlier that 'whole' method learning involves fear, hesitation, etc., throughout almost the entire learning period. This is naturally accumulative, as has been made clear in earlier chapters. Even when fear is almost eliminated, any disturbing factor, *e.g.,* a slight noise, will cause this injurious condition to operate again. The entire run may be affected. This disturbance often fails to subside for many days. The 'part' learner rarely if ever manifests such instability after one short maze has been mastered. So it seems as though no favorable emotional tone can operate as a transfer element in a long maze, because of the very complexity of the course and the many pitfalls and surprises involved.

Specific. The writer has little to say regarding specific transfer in the entire act. However it may have tended to operate, it seems unable to do so to any great degree, this fact being traceable mainly to the operation of injurious emotional conditions. For example, the duplicated cul de sacs—nos. 3, 6, 9, 12 in Maze A show far greater frequency in 'whole' method learning than in 'part'.

In summary, the writer is convinced that neither general nor specific elements of transfer can be utilized to any high degree in learning a complex motor problem when learning is by the 'whole' method. The demonstrated ability of these transfer items to operate at their full value in 'part' learning seems to the writer to be unmistakable evidence of the advantage of

'part' procedures. This, taken in conjunction with the progressive elimination of the positional factors established in 'modified part' learning, goes a long way towards explaining the favorable results obtained by the 'modified part' methods.

(b) Learning effort and length of material.

An additional aspect of learning a motor problem by short stages needs to be considered. The relation between the learning effort and the length of material needs to be known. Is there a law of diminishing returns operating that makes a long maze more than twice as hard to master as one only half the length? Data are at hand to answer the problem.

It has been shown that the units of Maze A are, in number of cul de sacs, length of true pathway, etc., exactly equal. Consequently, each of these is an equal fourth of the entire maze. No better motor situation could be desired for the comparison of learning effort and length of material. Even series of nonsense syllables furnish scarcely more equitable bases for comparison. The lists used are made equal in length and supposedly in difficulty. Roughly, the short maze sections seem to satisfy the same conditions. Even though these sections may differ in difficulty (as measured by the learning criteria), a comparison of each of them with the total maze will give results highly valuable. It is logically necessary to make this comparison for the entire four, since they are integral parts of the total problem under consideration. The three learning criteria may be brought together in the following formula,- $\frac{t'.\ s'.\ e'}{t.\ s.\ e}$, where t', s' and e' represent the records in trials, time and errors respectively of the control groups on Section I, II, III, or IV and t, s and e the corresponding records for the entire maze. Such a formula may weight to an unfair degree certain of the learning criteria. But it is far from being decided which criterion is the best, so the writer is inclined to utilize them all in one formula. If these criteria varied directly, such a formula would not be needed.

The records used are listed in Tables I, VI and X for the rats and Tables III, VI and XI for the humans. The records for the learning of the problem as a whole are those where

no more retracing was allowed than normally occurs in the learning of the separate parts ('whole' method with returns prevented).[1] For Section I the records of the 'part' learners are used. Control groups furnish the records for the remaining sections.

The results of the formulaic estimations are decidedly significant. For the rats it is found that 15, 1, 3 and 2 percent of the learning energy expended upon the entire maze is required to master units I, II, III and IV respectively. With the humans, the results are 4, 6, 1 and 11 percent. As a final average, the rats score 5.25 percent, the humans 5.5 percent. It is clear that this value would be 25 percent, provided that learning effort varied directly with the length of material. The figures argue that mazes of porportionate difficulty and of one-fourth the length of a larger maze, require scarcely more than one-twentieth the learning effort needed to master the larger problem.[2] This generalization holds for both rats and humans.

[1] Chapter III brought out the logical necessity of making comparisons with these data rather than with the results where freedom of retracing into the return sections was allowed. Of course the time and error records are far higher under the non-restricted conditions.

[2] Ebbinghaus has shown that there is no direct relationship between the length of nonsense series and the learning effort required. He presents two tables of data (*Memory*, pp. 47-49, Columbia University Teachers College Educational Reprints No. 3), these being secured during two testing periods separated by an interval of three years. They have special value in showing not only that diminishing returns for the learning effort operate but also that this loss (due to excessive length of the material) is not fully eliminated when practice effect is developed to its maximum. This is shown by bringing together the original data into one table and starring the results from the first testing series.

Number of syllables in a series	Number of repetitions necessary for first errorless reproduction (exclusive of it)
7	1
10	13*
12	16.6
13	23*
16	30
16	32*
19	38*
24	44*
36	55

The periodic regularity in which these results distribute is marked. The

The significance of these results for the 'whole'-'part' problem is clear. It pays—other things being equal—to learn a complex motor problem by easy stages. Otherwise, diminishing returns for the energy expenditure are secured. The causes of this need not be sought here, although they are probably inherent in the conditions of transfer discussed in the present chapter. Irrespective of causes, the facts of energy expenditure function well in explaining the results of 'modified part' procedure. The advantage of learning by easy stages, taken in conjunction with transfer conditions and the progressive elimination of the positional factors set up in 'part' learning, go a long way not only toward explaining the superiority of the 'modified part' methods over the 'whole' method, but also toward pointing out the inherent advantages of any 'part' procedure. There are probably other explanatory factors that have not been suggested. The above are not meant to be exhaustive. They do represent to the writer, however, the only explanations he can now set forward to meet a novel and exceedingly interesting experimental finding.

The following summary lists the important developments of the chapter:

(1) Transfer factors operate at their full value in 'part' procedure.

(2) This transfer is general and specific. The important general items are a general maze habit, consciousness of power, and favorable emotional tone. The specific items refer to the details of the maze pattern.

(3) Transfer fails to render the final areas of a complex motor problem more easily mastered. 'Part' procedure reverses these conditions.

(4) Learning effort does not vary directly with the length

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chief significance of these results for the present research rests, however, upon the fact that they, taken in conjunction with the results of the present research, make it possible to state that the law of diminishing returns operates (a) in the mental field, (b) for the learning of motor and verbal material, (c) for humans and animals, and (d) irrespective of earlier practice, though this is contradicted by Meumann.

of material. Diminishing returns are secured as the material is lengthened.

(5) The inherent advantages of 'part' learning are mainly the complete utilization of the transfer items and the avoidance of diminishing returns due to the excessive length of the motor problem.

(6) The inherent advantages of 'part' learning, together with the elimination of place associations, explain the universal superiority of 'modified part' methods in motor learning.

CHAPTER VII

Massed vs. Distributed Effort in 'Whole' and 'Part' Learning

The relation of the distribution of learning effort to the 'whole'-'part' discussion is obvious. Nothing totally new is being injected into the research. Heretofore the writer has considered the 'whole' and 'part' methods when two trials were allowed per day. Limiting the number of trials per day is necessary when rats are being employed for the experimentation. Consequently, the same time relationships had to be maintained for the humans, otherwise no comparative statements could be made. Under these defined learning conditions, it has been shown that the 'whole' method of learning is superior to the 'part' method but that the bad aspects of the 'part' method can be so eliminated as to produce 'modified part' methods far superior to the original 'whole' method. These generalizations have been shown to hold for both rats and humans. But it is obvious that no comparison can be made with any previous work on humans, where verbal material was used. Massed effort has always been used in the 'whole'-'part' testing, whether the learning was nonsense material, prose or poetry. Hence, the writer desires to find whether the relative value of the 'whole' vs. the 'part' methods depended to any degree upon massing or distributing the learning effort. With maze results established for massed learning conditions, comparisons might then be made with the verbal results.

The experimental literature contains numerous references to the value of distributed effort in motor learning. Browning, Brown and Washburn (2) early showed that such distribution was favorable. Murphy (12) has just published his results for javelin throwing. His conclusions are based upon the records of groups practicing one, three or five times per week. He is inclined to generalize for rote and logical learning as well as

for motor. "Better work, for the amount of time expended, can be done in our schools (both for hand manipulations and also so-called mental work), through a distribution of three times per week than through a distribution of five times per week." Ulrich showed that rats learned the maze with fewer trials when effort was distributed to trials every third day. No human maze experimentation has been published. No comparative experimentation has been done with rats and humans to test out the respective value of massed vs. distributed effort.[1] So far as regards the 'whole' and 'part' methods in relation to the distribution question, the literature shows that the matter has never been treated. The present chapter is concerned with this problem.

Six new groups of humans were secured. Each group included six subjects. Each group was taught Maze A by one of the methods described in earlier chapters. These in order were as follows: 'Whole', with returns allowed, 'whole', with returns prevented, 'total part', 'progressive part', 'direct repetitive', and 'reversed repetitive." Preliminary instructions, methods of data gathering, etc., were the same as in previous experiments. The sole difference was that no time was allowed to elapse between trials.[2] As soon as a run was completed, the subject was given subsequent trials, with no time for rest, conversation, removal of the hand from the maze area, etc. Continuous attack upon the learning problem was absolutely secured.

The behavior of these groups merits some comment. The

[1] Obviously, a problem would need to be relatively simple to permit mastery by the rat in successive trials. The initial emotional complex, constant distractive tendencies, etc., render massed tuition extremely difficult. However, with a simple problem, thorough preliminary training, utilization of hunger, sex and other stimuli, the rat may be taught a motor problem without the customary time interval. At least the learning could be concentrated within a few hours.

[2] Perrin's recent comparative work on adult and child maze learning allowed "all the time between trials they desired for rest, physical and mental recreation." Such is no doubt needed with the children. It is a question, however, whether conclusions may justly be drawn between individual children or between children and adults if the time interval is not constant as to length.

group learning the maze as a whole and with no prevention of returns attacked the problem well. After a few trials, improvement ceased and the learning act became very trying. A long period of almost random activity, hurried yet purposeless exploring ensued. Each student showed signs of nervousness when the first dozen trials failed to bring success. To this excitability the men were even more susceptible than the women. As each successive trial continued to bring errors, the run became more hurried, jerky and erratic. Generally the student reached a stage in his learning where he knitted his brow, closed his eyes, checked his speed and settled down to a slow, laborious process of eliminating certain specific errors per trial. The period of high tension remained and the completion of the final successful runs always brought unmistakable relief. The 'whole' method with returns prevented produced behavior such as the above, yet this was to a lesser degree. Without exception, the 'part' methods *failed* to arouse a strong emotional tone or to cause nervous excitement. The attack upon the problem was always steady, irrespective of the method employed. The act of connection in 'part' learning was singularly free from confusion, this being a very marked departure from the behavior so characteristic of both humans and rats when learning was broken by the twenty-four hour interim. The data of these experiments in massed effort are compared with those of the humans when effort is distributed in Table XXI. In Table XXII is found the percentage of advantage or disadvantage obtained by massing effort. The table is complete for trials, time, and errors of the various types. See pages 77, 78.

In agreement with the previous conclusions, massing the learning effort is highly unfavorable for certain methods. It increases the number of trials, the learning time, and all types of errors when the 'whole' method of learning is used. This is true irrespective of the prevention of returns and applies to all measuring criteria. Without exception there is a marked percentage of gain when distribution occurs. This is true also for two 'part' methods—the 'direct repetitive' and the most efficient for all human and animal learning under distributive

conditions, namely the 'progressive part'. The advantage is, however, less marked than for 'whole' method learning. In some items of comparison, the absolute differences are almost negligible. Significant changes occur so far as the total 'part' method and the 'reversed repetitive' are concerned. By all measuring criteria, the 'part' method gains over distributive results and the same is true for the 'reversed repetitive' (with slight and unimportant exceptions for this latter.)

But the significant results are not so much in reference to the comparison of the same method under these changed temporal conditions as to the important fact that each 'part' method shows superior (by all criteria of measurement) to either type of 'whole' method, thus altering very markedly the 'whole'-'part' results set forward in earlier chapters. Furthermore, the total 'part' method (so unsuccessful under distributive conditions) under massed conditions becomes not only better but almost the best of all available methods. It is even slightly more efficient than the universally superior 'progressive part' in point of trials but slightly weaker in time and considerably so in total errors. Consequently it is considered second in advantages. *But its rise in the efficiency scale under these massed conditions is unmistakable.* Its great gain consisted not so much in the learning of the four units but in the connecting act. For Section I the total error record was greater than in the distributed case, the remaining three being almost identical. (24, 12; 10, 9; 5, 4; 7, 5 for I, II, III, IV respectively). The change of results for the 'part' method are so marked that a full comparison of the data is made in Table XXIII. The difference in scores is especially obvious in the I-IV act of connection. See page 78.

It is evident that explanation is needed for these massed effort results. It needs to be shown why each type of 'part' learning is superior to that of the 'whole'. These results are not only opposed to those secured under distributive conditions but exactly contradictory to the ones commonly accepted for verbal learning. In the latter, learning effort is always massed, yet herein have the results always been in favor of the 'whole' method procedure. The writer can do little more than speculate.

Until the problem of massed effort is understood and its relationship not only to distributed effort but also to different types of problems established, final conclusions directed towards the 'whole'-'part' procedure are impossible. Each is a separate problem, yet each depends upon and illuminates the other.

Learning of the maze problem passes through two distinct stages. (a) *Elimination.* The subject, after getting acquainted with the general character of the maze, settles down to the arduous task of eliminating all types of errors. The enormous time expenditure and the great number of errors made during the first half of the tuition period point out the difficulty of this learning act. Increasing complexity of the maze brings increasing demands upon the subject. These specific demands have been commented upon at length in earlier chapters. Now, with massed effort the confusion is cumulative from trial to trial. In place of the errors being gradually eliminated, instability is generated. Through this period there is little chance that the needless movements of one trial will fail to appear in the ones directly following. Even their disappearance for one trial argues little for their permanent loss. It is during this stage of discovery and elimination that distributed effort has its place. The many useless movements tend to fall away from the success-bringing series, while the latter seems to affect the serial bonds during the interim of inactivity. With rats and humans, the measured improvement is, from day to day, too commonplace to warrant comment.

(b) *Mechanization.* Logically following the preceding but chronologically inseparable from its final stages, is the period where the subject is *hammering in* the final, sensori-motor coordinations. Tendencies to enter cul de sacs, to retrace, etc., are still present but these are swamped in the rapid, forward-going activity. The function of this period is to render definite the elimination of these errors and to increase the speed of the run. Exploration has now no place. The activity is well on its road to the habitual level. Its momentum is its major guarantee of success. This is the time for massed effort. By successive repetitions of the successful runs, the tendencies to error

become less and less liable to function, as the fixation proceeds rapidly and surely. In a strictly psychological sense, the subject who hesitates is lost. He can now drive out of the series the danger-giving elements and so render their elimination permanent. In distributing his effort, such permanent elimination is certainly less slow in attainment. Efficiency demands massing of the learning.

The principles of elimination and mechanization have immediate applicability to the 'whole'-'part' learning problem. They show at once, no doubt, why any form of the 'part' method is superior to the 'whole' under massed conditions. The parts as such were always simple. Hence, the need for distribution of learning effort was reduced to a minimum. (It seems logical that distributive and massed efforts should be equally efficient for simple mazes. This needs to be shown experimentally). However, even in our simple I-IV sections, there was slight advantage with distribution. (See figures, p. 62.) The act of connection demanded speedy, non-exploring, rapidly succeeding attacks. Massing the effort provided this, hence making all the 'part' methods highly efficient. With the 'whole' methods there was no opportunity for the great number of useless movements to disappear automatically during the time interval. Rather, they remained to mar the runs, to delay final success, and increase the nervousness of the subject. Only by the greatest effort were they finally eliminated. If the subject had been able to break up the task of error learning into simple, easily mastered units, to eliminate errors and mechanize each unit, and to expend his best energies in rapid attacks at connection, success would have been far more quickly attained.

It appears, therefore, that reliance upon massed or distributed effort depends somewhat upon three fundamental factors. (1) *Difficulty of the problem.* The problem with many possibilities of error needs distributive handling. This need decreases with decreasing difficulty of the problem. (2) *Stage of the learning.* In the discovery and eliminating stages, distribution is essential. In the stage of strenuous mechanization, massing of effort is advisable. (3) *Method of learning.* The 'whole' method re-

quires distribution for easy mastery, the 'part' methods may require distribution for mastery of the units but massing for connection. Under massed conditions the 'part' methods are always more efficient.

These principles seem fundamental for the human pencil maze situation. They may need expansion or qualification. They are put forward as suggestive and with the hope that experimentation may be directed toward a problem regarding which there is practically no knowledge. It may be that the results for learning verbal material under massed and distributed conditions must be recanvassed. It is clear that one or more of four conditions must maintain. (1) The conclusions of the writer are not truly descriptive of the specific motor problem discussed. (2) These results, though true for the maze, are not general for the entire motor field of learning. (3) The results in verbal learning (for all types of material) need reconsideration. (4) There are no correlations to be drawn between learning upon the motor and ideational levels. The writer cannot agree to the accuracy of (1). He is inclined to argue from his results to the general motor field (2). Any opinion regarding (3) and (4) is sheer speculation, whose validity must rest upon experimental results.

The results of this investigation of the distribution of learning effort in relation to the 'whole' and 'part' methods of learning may be summarized as follows:

(1) Massing the learning effort is highly unfavorable for 'whole' method learning. This is in agreement with the accepted results in the experimental field.

(2) The pure 'part' method proves much more efficient than when effort is distributed.

(3) All types of 'part' methods produce better learning results under massed conditions than do the 'whole' methods. This superiority is demonstrated by all measuring criteria.

(4) The superiority of the 'part' methods are probably statable in terms of the eliminative and mechanizing aspects of the learning period.

(5) Reliance upon massed or distributive effort depends upon

a number of fundamental factors. Chief of these are the difficulty of the problem, the stage of the learning, and the method of learning.

(6) Learning a motor problem by 'part' methods produces results that contradict the findings secured under like massed conditions with rote and logical material.

(7) The full significance of the distribution of the learning effort is far from being known.

CHAPTER VIII.

Comparison and Summary

The conclusions of this experiment have been cumulative. They have been discussed at length in the body of the paper at their point of emergence. It is here merely in order to state the final conclusions regarding the propositions formulated in Chapter I.

I. Efficiency of various 'whole'-'part' learning methods in the motor situation.

a. The 'whole' method with returns prevented is more efficient than with returns allowed.

b. The 'whole' method is far more advantageous than the 'part' method.

c. The 'whole' method is decidedly less favorable than the 'progressive part' and 'direct repetitive' part methods. With the rats, the 'reversed repetitive' part methods. is also more efficient. Failure for this to prove so with the human is due to the inability of the kinaesthetic cue to function as the recognitive agent.

d. The weaknesses of the 'part' method are not due to negative transfer in the learning of the motor units, disintegration through time, retro-active inhibition, contiguity of unit functioning, nor unit incompatibility in a larger series. The weaknesses are due to failure in the act of connection, the conditioning factors being traced to the positional aspects of the temporal and spatial series.

e. 'Part' procedure possesses certain inherent advantages. These are mainly the complete utilization of the transfer items and the avoidance of diminishing returns due to the excessive length of the motor problem.

f. The strength of all types of improved ('modified') part methods rests upon the progressive elimination and distributive handling of the emotional and positional factors, together with the inherent advantages of any 'part' procedure.

II. Universality of various 'whole'-'part' learning methods in the motor situation.

a. Improvement of 'whole' method learning universally follows when returns are prevented.
b. The pure 'part' method is universally inferior to the 'whole' method.
c. The 'progressive part' and 'direct repetitive' methods are universally far more advantageous than the 'whole' method. The 'reversed repetitive' method fails to attain like universality, owing to the recognitive inefficiency of kinaesthesis in the human. In general, however, almost any type of 'modified part' method is universally to be preferred.
d. For all methods the correlations between the various learning criteria of trials, time and errors are universally high. No royal road in motor learning is open to the human and denied to the rat. Certain changes in position for certain methods render a cross correlation less marked. These shifts are traceable to differences in the retracing tendency and the recognitive capacity. They do not vitiate the comparative results listed in a-c above.
e. The pure 'part' and 'modified part' methods become increasingly superior to the 'whole' method when learning effort is massed rather than distributed.

III. Comparison of motor learning and learning verbatim.

(1) Comparison of methods.

a. The 'whole' method with returns allowed is the N-Verfahren or "natural" method (Steffens).
b. The 'whole' method with returns prevented is the G-Verfahren (Steffens), Das Lesen im ganzen (Ephrussi), Lernen im ganzen (Pentschew, Meumann, etc.), Methode globale (Larguier des Bancels) and the standardized 'whole' procedure of the English investigators (Lakenan, Pyle and Snyder, etc).
c. The pure 'part' method is the second S-Verfahren (Steffens), Das Lesen mit gehaüften Wiederholungen (Ephrussi), Lernen in Gruppen (Pentschew), Methode fragmentaire (Larguier des Bancels) and the 'part' method of the English experimentation (Lakenan, etc).
d. The 'progressive part' method resembles to a slight degree the second part method of Pyle and Snyder.
e. The 'direct repetitive' method resembles to a slight degree the first S-Verfahren (Steffens). The 'reversed repetitive' method finds no method comparable to it.
f. The 'elaborative part' method resembles to a slight degree the first part method of Pyle and Snyder.

g. No motor method employed is comparable to the Lernen im gebrochenen ganzen (Pentschew).

(2) Comparison of results in motor learning and learning verbatim.

a. The 'whole' method with returns allowed agrees with the "natural" method in learning verbatim as being very inefficient.

b. The 'whole' method with returns prevented agrees with the 'whole' method in learning verbatim as being superior to the "natural" method and the pure 'part' method, (waiving the single exception of Ephrussi's conclusions for learning nonsensical material by the 'part' method).

c. The 'progressive part', the 'elaborative part', and the 'repetitive part' methods, though proving superior to the 'whole' method in motor learning, fail to do so in learning verbatim. However, these motor methods have not been strictly duplicated in learning verbatim (either with rote or logical material), so an exact comparison is unwarranted.

e. The several favorable modifications of the 'part' method as employed in motor learning, need to be tested in learning verbatim. Until such be done, it seems unwarranted to argue that all types of 'part' methods are inferior to the 'whole' method for the learning of rote and logical material.

IV. Relation of the conculsions to practical schoolroom activities of the motor type.

a. The complex motor problem is probably always best mastered by one of the several 'modified part' methods. The one universally to be preferred is the 'progressive part'.

b. Distribution of the learning effort is of value for the 'whole' method but not for the 'part' procedure.

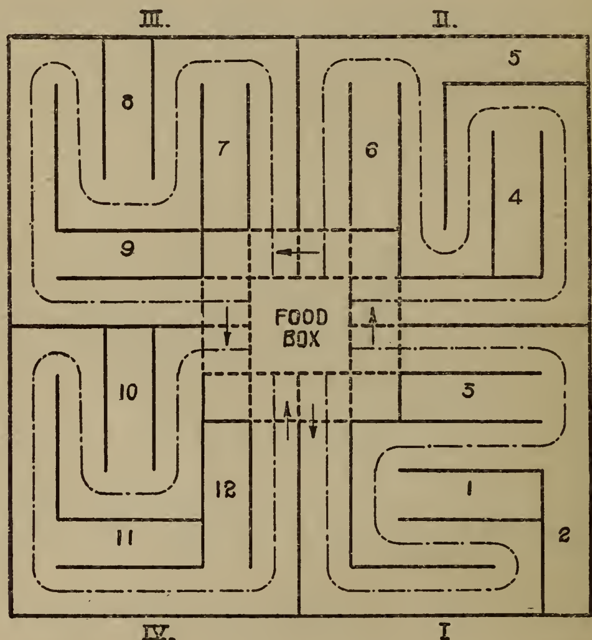
c. Distribution of the learning effort is of value for the exploring and eliminative stages of learning, not for the rapid mechanizing stage. Here effort should be massed.

d. When the conditions of learning call for a massing of learning effort, the 'whole' method becomes increasingly inefficient with increase in problem complexity, the 'part' methods increasingly more efficient.

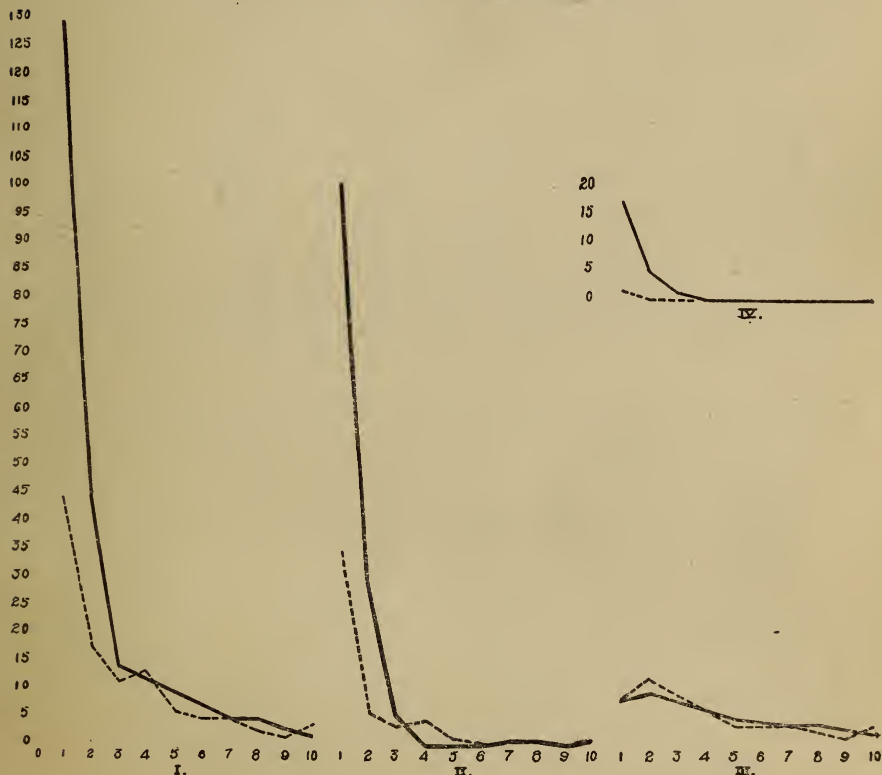
e. The conclusions drawn apply solely to the motor type of learning, though they suggest that the rote and logical types need additional experimentation.

APPENDIX

Figure 1. Maze A. Roman numerals refer to the four independent sections of the maze. Dotted division lines indicate the entrances and exits for the different sections into the food-box. Also, they designate the removable panels. The running dotted lines show the true pathway for each section. The arrows between sections point out the continuous pathway when the maze is being learned as a whole or in the connection of the separately learned units. The vertical arrows indicate the main entrance and exit. Arabic numerals designate the cul de sacs. Slides for the prevention of returns occur after cul de sacs numbered 3, 6, 9, 12.



Graphs I-IV. To show graphically the learning curves of two rat groups in learning Maze A with and without the prevention of returns. The unrestricted group had 12 rats, the restricted 9. The former is represented by the solid line, the latter by the dotted. No. 1 is the total error curve; no. II for the retraces (Type C); no. III for the forward cul de sacs (Type A); no. IV for the retrace cul de sacs (Type B). See Table VI. (The learning is divided into ten equal stages as based on the number of trials and the errors of each tenth of the learning computed. See the method of Vincent, The Function of the Vibrissae in the Behavior of the White Rat, Behavior Mon., 1, 5, 1912, pp. 15-17.)



	Trials	Time	Errors			
			A	B	C	Total
I	34	470″	37	2	13	52
II	2	33	1	0	2	3
III	14	127	9	0	5	14
IV	9	111	8	0	3	11
I-IV	15	1166	19	15	85	119
Total	30	1907″	74	17	108	199

Table I. A table to show the average number of trials, time and errors of nine rats in learning Maze A by the 'part' method. In estimating the total number of runs, each sectional run is arbitrarily counted as one-fourth of a complete run.

	Trials	Time	Errors			
			A	B	C	Total
Whole	27	4174″	54	24	139	217
Part	30	1907″	74	17	108	199

Table II. A table to compare the average number of trials, time and errors of two rat groups learning Maze A by the 'whole' and 'part' methods respectively.

	Trials	Time	Errors			
			A	B	C	Total
I	6	198″	4	0	20	24
II	3	47	3	2	5	10
III	2	49	1	0	4	5
IV	1	25	1	1	5	7
I-IV	20	901	28	22	141	191
Total	23	1220″	37	25	175	237

Table III. A table to show the average number of trials, time and errors of seven humans in learning Maze A by the 'part' method.

	Trials	Time	Errors			
			A	B	C	Total
Whole	12	641″	16	13	97	126
Part	23	1220″	37	25	175	237

Table IV. A table to compare the average number of trials, time and errors of two human groups, learning Maze A by the 'whole' and 'part' methods respectively.

	Trials	Time	Errors			
			A	B	C	Total
Rats						
Whole	27	4174″	54	24	139	217
Part	30	1907	74	17	108	199
Humans						
Whole	12	641	16	13	97	126
Part	23	1220	37	25	175	237

Table V. A table to compare the averages of rats and humans in 'whole' vs. 'part' learning. These data are extracted from Tables I-IV.

	Trials	Time	Errors			
			A	B	C	Total
Rats						
Allowed	27	4174″	54	24	139	217
Prevented	30	1666	56	4	51	111
Humans						
Allowed	12	641	16	13	97	126
Prevented	17	541	23	6	51	81

Table VI. A table to compare the average number of trials, time and errors of rat and human groups, learning a motor problem (Maze A) as a whole, with returns allowed and prevented.

	Trials	Time	Errors		
			A and B	C	Total
Allowed	50	2886″	188	124	312
Prevented	50	1813″	151	53	204

Table VII. A table to show the average number of trials, time and errors of two rat groups given fifty trials upon Maze B, with and without the prevention of returns.

	Percentage of Group	Trials	Time	Errors			
				A	B	C	Total
Rats							
Allowed	64	36″	2676	155		119	274
Prevented	54	33	1818	116		55	171
Humans							
Allowed	100	33	2599	101	155	407	663
Prevented	100	51	2669	130	130	388	648

Table VIII. A table to show the average learning record of rat and human groups upon Maze B with and without the prevention of returns.

	Trials	Time	Errors A	Errors B	Errors C	Errors Total
Rats						
Whole	30	1666″	56	4	51	111
Part	30	1907	74	17	108	199
Humans						
Whole	17	541	23	6	51	81
Part	23	1220	37	25	175	237

Table IX. A table to compare the average number of trials, time and errors of rat and human groups, learning Maze A by the 'whole-prevented' and 'part' methods.

		Trials	Time	Errors A	Errors B	Errors C	Errors Total
Sec. II	Control Group	8	128″	6	1	6	13
	Part Learners	2	32	1	0	2	3
Sec. III	Control Group	20	254	19	2	14	35
	Part Learners	14	127	9	0	5	14
Sec. IV	Control Group	9.25	316	7	2	21	30
	Part Learners	9	111	8	0	3	11

Table X. A table to compare the average learning of rat groups upon a single maze (control group) with the averages for the group having previously learned one or more mazes (Part Learners).

		Trials	Time	Errors A	Errors B	Errors C	Errors Total
Sec. II	Control Group	5	132″	2	2	7	11
	Part Learners	3	47	3	2	5	10
Sec. III	Control Group	8	183	7	6	19	32
	Part Learners	2	49	1	0	4	5
Sec. IV	Control Group	10	254	4	4	43	51
	Part Learners	1	25	1	1	5	7

Table XI. As for Table X, human learning.

	Trials	Time	Errors			
			A	B	C	Total
I	.4	2.5″	.4	0	0	.4
II	0	0	0	0	0	0
III	2	13.4	1	.2	1	2.2
IV	0	0	0	0	0	0
Average	.6	3.9″	.35	.05	.25	.65

Table XII. A table to show the average relearning effort of a group of rats having been taught subsequent motor habits after mastering earlier ones. The data are indicative of retro-active inhibition.

	Trials	Time	Errors			
			A	B	C	Total
I & III	.2	6.3″	0	.1	1	1.1
I-IV	0	0	0	0	0	0
II & IV	.3	5.3	.33	0	0	.33
I-IV	0	0	0	0	0	0
IV & I	4	10	.2	.3	1.5	2

Table XIII. A table to show the average records of a rat group in the elimination and subsequent reconstruction of specific motor units learned as parts of a larger motor situation (Maze A).

	Trials	Time	Errors			
			A	B	C	Total
I & III	2	12″	1.25	0	.5	1.75
I-IV	0	0	0	0	0	0
II & IV	.5	3″	.50	0	.25	.75
I-IV	1	24″	.5	.25	.5	1.25
IV & I	.5	10″	.25	0	1.5	1.75

Table XIV. As for Table XIII, human group record.

	Time Interval	Trials	Time	Errors			
				A	B	C	Total
I	15 Days	.17	1″	.17	0	0	.17
II	8 Days	1	7	1.33	0	.5	1.83
III	5 Days	.17	.67	.17	0	0	.17
Average	9.33 Days	.45	2.89	.56	0	.17	.72

Table XV. A table to show the disintegration through time of the control upon the various maze sections as mastered in part learning, based on the average for six rats.

	Time Interval	Trials	Time	Errors			
				A	B	C	Total
I	13 Days	0	0	0	0	0	0
II	8 Days	1.5	9.3″	.83	0	0	.83
III	5 Days	1.3	10.1	1	0	1	2
Average	8.67 Days	.93	6.5	.61	0	.3	.94

Table XVI. As for Table XV, human group records.

Method	No. of Rats	Trials	Time	Errors			
				A	B	C	Total
Progressive Part	9	11	662″	39	2	24	65
Reversed Repetitive	8	17	882″	22	5	49	76
Direct Repetitive	11	21	1442″	45	9	88	142
Whole Returns Prevented	9	30	1666″	56	4	51	111
Total Part	9	30	1907″	74	17	108	199
Whole Returns Allowed	12	27	4174″	54	24	139	217

Table XVII. A table to show the average group records for the learning of Maze A by standard and original methods. In estimating total trials for these methods, each section traversed is counted quite arbitrarily as one-fourth a run. This probably weights the runs through the mastered sections but never in such a way as would produce more favorable comparisons with the 'whole' or pure 'part' methods. The methods are listed in their apparent order of merit, although the three measuring criteria do not always agree in arguing for this order.

Method	No. of Humans	Trials	Time	Errors			
				A	B	C	Total
Progressive Part	6	10	352″	10	3	44	57
Direct Repetitive	6	11	618	15	11	70	96
Whole Returns Allowed	6	12	641	16	13	97	126
Whole Returns Prevented	6	17	541	23	6	51	81
Reversed Repetitive	6	22	1014	27	24	175	226
Total Part	6	23	1220	36	25	176	237

Table XVIII. As for Table XVII, human learning.

Trials	Time	Errors			
		A	B	C	Total
1	41″	.4	.2	3.4	4
2	26″	.2	0	0	.2
3	33″	.4	0	0	.4
4	24″	0	0	0	0

Table XIX. A table to show the average time and errors per trial of six rats in connecting four sections, such connection having been preceded by an increasingly complex review of the various units. This is the nearest rat approach to massed learning effort.

	Trials and Time	Trials and Total Errors	Time and Total Errors	Trials and Type A Errors	Time and Type A Errors	Total and Type A Errors
Rats	.8705	.8325	.9451	.9269	.7750	.6775
Humans	.8325	.8325	1.0000	1.0000	.8325	.8325

	Trials	Time	Total Errors	Type A Errors
Rats and Humans	.6180	.3335	.3335	.4465

Table XX. A table to show the correlation between the learning criteria for all methods, both for rats and humans. Also, the correlation between the rat and human learning for all methods, correlation being measured by a single learning criterion. Ranking method $P=1-\frac{6\Sigma D^2}{n(n^2-1)}$

Method	Trials	Time	Errors			
			A	B	C	Total
Whole-Returns Allowed	30	1250″	41	27	192	260
	12	641	16	13	97	126
Whole-Returns Prevented	25	1208	40	14	150	204
	17	541	23	6	51	81
Total Part	10	538	15	9	83	107
	23	1220	36	25	176	237
Progressive Part	14	536	24	10	62	96
	10	352	10	3	44	57
Direct Repetitive	24	716	37	7	76	120
	11	618	15	11	70	96
Reversed Repetitive	20	764	29	10	87	126
	22	1014	27	24	175	226

Table XXI. A table to compare the average learning records of human groups for the various methods, with effort being massed and distributed. The results for massed effort always appear first.

	Trials	Time	Errors			
			A	B	C	Total
Whole-Returns Allowed	150	95	156	98	108	106
Whole-Returns Prevented	47	123	74	133	194	152
Total Part	−57	−56	−58	−56	−53	−55
Progressive Part	40	52	140	233	41	68
Direct Repetitive	118	16	147	−36	9	25
Reversed Repetitive	−9	−25	7	58	−62	−44

Table XXII. A table to show the percentage of advantage or disadvantage of massed in comparison with distributed effort.

Section	Trials	Time	Errors			
			A	B	C	Total
I	5.4 6	93" 198	2 4	2 0	8 20	12 24
II	1.2 3	45 47	1 3	1 2	7 5	9 10
III	2.4 2	28 49	3 1	0 0	1 4	4 5
IV	2.4 1	28 25	1 1	0 1	4 5	5 7
I-IV	7.6 20	344 901	8 28	6 22	64 141	78 191
Total	10 23	538 1220	15 37	9 25	84 175	108 237

Table XXIII. A table to compare the average learning records of human groups for the 'part' method, with effort being massed and distributed. The results for massed effort always appear first.

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EDWARD HERBERT CAMERON, PH.D.
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A study (1) of the effects of practice on singing tones at two different pitch levels near the upper and lower limits of the range of the male voice; and (2) of the relation of these activities to discrimination of tones.

Practice in singing a tone of a certain pitch level resulted in improvement in accuracy of pitch of the tone in the cases of four out of six subjects. This improvement was not present in the singing of tones at a different level from that in which practice had taken place. Practice in singing tones of a certain pitch was followed by improvement in discrimination of tones of the same pitch but no improvement in discrimination took place at the other pitch level. Improvement in the *uniformity* of the practised sung tone was transferred to the other level.

Perhaps in no other sphere is the usual voluntary response to a sensory stimulus more precise and unvarying than in the reproduction of tones by singing. The vocal production of tones is, therefore, peculiarly adapted for the study of sensori-motor habits. While the sensory processes involved in such habits have been studied in detail, relatively little investigation has been made of the related motor processes. In a former paper the writer has shown some of the characteristics of sung tones and their changes under varying conditions of attention. Berlage (1) and Miles (7) have confirmed this work in some respects and expanded it in others.

The object of the investigation now presented was fourfold:

(1) To determine the relationship between ability to discriminate tones of a certain pitch and the ability to sing tones of the same pitch.

(2) To determine the changes which take place in the singing of tones of a certain pitch during the course of a long practice series.

(3) To determine what influence, if any, practice in singing

of a certain pitch has on the ability to discriminate tones (a) of the same pitch and (b) of a different pitch.

(4) To determine what effect, if any, practice in singing tones of a certain pitch has on singing tones of a different pitch.

For carrying out such an investigation it is necessary to have (a) a means of testing sensory discrimination of tones of different pitch; (b) a means of recording the pitch of a sung tone.

The difficulties of a method for the former purpose are well-known. Seashore's report (10) recommends the use of standard tuning forks of slightly different pitches and appropriate resonators. This method was not used in the present instance because of difficulty in obtaining forks of the range of the human (male) voice, and because of certain advantages of manipulation to be gained by the method actually adopted.

In the method used the source of the tones, both for purposes of testing discrimination and for reproduction by singing, was electric tuning forks. Four such forks, two of approximately 100 v. d. and two of approximately 225 v. d. frequency were used. These forks were placed in circuit with a single telephone receiver of the watch case variety, which was placed in a room adjoining that in which the forks were placed. The prongs of one of each pair of forks were fitted with adjustable weights by means of which the pitch was varied above or below that of the other fork of the same range, the pitch of the latter being kept constant. The variations in pitch were measured indirectly by noting the position of the weights on a millimeter scale attached to the base at one side of the fork. To facilitate the reading of the position of the weights one of them was provided with a pointer which projected to a point just above the scale. The pitch for the various readings on the scale was ascertained by placing the forks in circuit with an electric marker recording on a smoked paper belt.

The telephone and forks were connected by means of a Pohl commutator, the cross bars at the base having been removed. The semi-circular rods were kept out of the mercury cups when at rest by a pair of springs attached to the base. When the switch was in this position the circuit to the telephone was inter-

rupted. The current passed through the telephone and the standard fork when the free ends of the rods of the commutator were pressed in one direction, and through the telephone and variable fork when pressed in the opposite direction.

In the earlier investigation mentioned above there was used for the recording of sung tones a telephone receiver with mica diaphragm to which were attached magnifying levers. This method was discarded in the present investigation. A graphic record of the sung tone was obtained through the use of an ordinary voice key, similar to the Cattell model, but without any electro-magnetic attachment. The diaphragm is of very thin mica and is readily thrown into vibration by a tone sung into the air chamber. The voice key was placed in circuit with an electric marker of the Deprez pattern.

The sung tones were recorded on a long smoked paper belt running on two drums which were turned by hand. The time was recorded by an electric marker in circuit with a Kronecker interrupter regulated to intervals of 100 sigmas duration.

Before beginning the experiment the following preliminary tests were made:

(1) A test of the capacity of the telephone to register correctly the tone of the fork and respond to slight variations of tone. For this purpose recording levers were attached to the diaphragm of the telephone and the record thus obtained was compared with one obtained simultaneously from the fork through an electro-magnetic marker. There was no discoverable difference between the vibration rate of the fork and that of the telephone when recorded in this way.

(2) A similar test was made of the apparatus for recording the sung tones. A mica disk was attached to one of the prongs of the fork and placed in front of the air chamber of the voice key. In this manner records could be obtained of the vibration rate of the fork when transmitted through the voice key and comparison of these records with the records obtained at the same time through an electric marker showed no appreciable difference.

The tones obtained from the forks through the medium of the

telephone were not pure tones and differed somewhat in quality, especially in the case of the pair of forks of lower pitch. The intensities of the tones of each pair of forks were apparently the same. The qualitative differences did not remain constant, but varied with differences of adjustment of the forks, especially differences in the distance of the adjustable button from the spring of the fork. In order to ensure the best possible adjustment, the experimenter made use of a second telephone which could be connected with the forks by means of a switch. In this way he could test the tones at will and introduce such variations in adjustment as to secure better tones, when necessary.

At best, however, the qualitative differences were nearly always sufficient to enable the observer to distinguish the two tones. Great precautions had, therefore, to be taken in the discrimination tests to make sure that the results were due to differences in pitch and not differences in quality. Furthermore, on these and other grounds, all claims that the results give an absolute pitch limen for the subjects tested must be abandoned. It was sought only to obtain a limen which should hold under the conditions of this experiment and which should serve as a basis of comparison between the various observers who were tested.

The subjects who took part in the investigation were as follows: Professor Angier and Dr. Frost of the Yale Psychological Laboratory; Dr. Metcalf, laboratory assistant; and Messrs. Reed, Molby and Avey, graduate students in psychology. These subjects will be referred to hereafter as A., F., M., R., My., and Ay. respectively. All of these subjects except A. are of average or slightly more than average musical ability. M. and R. have had some training in the use of musical instruments, while My. and Ay. have been trained in singing to a slightly greater degree than the average person. A. and F. have had no musical training whatever. F. sings only infrequently. A. belongs to that class of individuals who cannot "carry a tune" correctly, though his inability in this respect is not as marked as in many cases.

All six subjects were given a preliminary test (1) of discrimination of pitch at both the higher and lower levels; (2) of ability to reproduce, by singing, tones of both the higher and lower

levels. Three of the subjects, A., F., and Ay., were then practised for a long time in singing the higher tone; the other three subjects, M., R., and My., were practised in the singing of the lower tone. After the practice in singing had stopped all subjects were re-tested as in the preliminary stage of the experiment.

In determining the capacity for pitch discrimination the following procedure was adopted. The observer sat in a partially darkened room with the telephone pressed lightly to the ear. A ready signal was given by means of a momentary flash of light from an electric lamp which was placed in front of the subject, but at some ten feet distance. A moment later the standard tone and the comparison tones were given for periods of two seconds each with a two second interval between them. The observer made known his judgment by pressing a key which was connected with a sound hammer in the experimenter's room. Judgments were classified as (1) same or doubtful; (2) higher; or (3) lower.

The pitch of the variable fork was varied by gradual steps below that of the standard fork and the threshold was regarded as having been reached at the point where the observer gave five correct judgments in succession. To avoid as far as possible unintentional judgments on the basis of the differences in quality of the two tones the standard tone was frequently given after the comparison tone. Furthermore, when it appeared that the threshold had been reached, the pitch of the variable fork was occasionally made higher instead of lower than that of the standard. In this way the experimenter could test whether the observer was meeting the requirements of the experiment by relying solely on differences of pitch in making his judgments. If it was found that this was not the case, as happened in several instances, the observer was cautioned and usually adopted a more critical attitude toward his judgments. Before recording any results in these experiments two periods of about twenty minutes each were allowed each observer for adaptation to the conditions of the experiment. The results of the preliminary test of discrimination of pitch follow (Table I).

The table gives the discrimination thresholds in vibrations per

second at both lower and higher pitches for each of the six subjects. Five trials on different days were made for each subject. In most cases the subjects expressed themselves as having no difficulty in distinguishing between the pitch and qualitative differences of the tones. In some instances, however, there was so much confusion that the test for that day was abandoned. While the confusion manifested may have been aggravated by the qualitative differences in the forks, it cannot be wholly charged to this cause, since other investigators have found similar results under more ideally perfect conditions.

TABLE I

Discrimination—Threshold before practice.

	Low (S = 100.3 V.D.)						High (S = 225.1 V.D.)					
	A.	F.	Ay.	M.	R.	My.	A.	F.	Ay.	M.	R.	My.
1.	1.9	1.9	1.9	1.3	1.3	1.9	3.2	2.6	2.6	2.0	2.6	2.6
2.	2.4	2.4	1.9	1.9	1.3	1.9	3.2	2.6	2.0	2.0	2.6	2.0
3.	2.4	1.9	2.4	1.3	1.3	1.9	3.8	2.6	2.0	2.0	2.6	2.0
4.	2.4	2.4	2.4	1.3	1.3	1.3	3.8	2.6	2.0	1.5	2.6	2.0
5.	1.9	2.4	2.4	1.9	1.3	1.9	3.2	2.6	2.0	1.5	2.6	2.0
Av.	2.2	2.2	2.2	1.4	1.3	1.8	3.4	2.6	2.1	1.8	2.6	2.1
A.D.	0.2	0.2	0.2	0.3	0.0	0.2	0.3	0.0	0.2	0.2	0.0	0.2

It will be seen from the table that the thresholds are greater for the higher than for the lower level. The average for all subjects at the lower level is 1.9 and for the higher level 2.4. These results agree with those of other investigators who have found that the threshold for pitch discrimination at various ranges does not increase proportionally to the vibration rate of the tones.

It will be noted that A., who is the decidedly unmusical subject, gives a record for pitch discrimination which does not differ largely from the records of the other subjects. At the lower level his average discrimination is the same as that of the other subjects. A.'s discrimination at the higher level is the highest of the six records, but it is not greatly different from that of two of the others.

In carrying out the experiments in singing the subject was given a ready signal (light) and a moment later the tone of the standard fork was sounded for two seconds. The subject was instructed to wait a moment after the tone stopped sounding and then sing into the mouthpiece a tone of the same pitch and ap-

proximately the same duration. Berlage and Miles have found that the pitch of a sung tone is somewhat modified by the vowel sound used in singing. The singer was, therefore, instructed to modulate his voice to the vowel sound "a" as in "ah." After allowing a few trials for the sake of adapting the subject to the condition of the experiment, five records were obtained from each of the subjects for each of the two tones. The records were read by means of the apparatus described in the former paper and in a similar manner to that of the earlier investigation. The results, expressed in vibration rate per second for each succeeding period of a tenth of a second's duration, are given in Table II. Figures are given for only the first second during which the tone was sung as this period is fairly representative of the whole tone.

In the column marked pitch is given the vibration frequency per second of each tenth of second for a continuous period of one second from the beginning of the tone. The figures given represent, therefore, the actual number of vibrations on the record multiplied by ten, so as to express the pitch for each period of 100 sigmas in the conventional unit of number of vibrations per second. Since the first two or three-tenths of second of the tone are marked periods of adjustment they do not represent the main tone correctly. Two averages are, therefore, given; the first for the earlier two or three-tenths of second, and the second for the remainder of the tone. An examination of the figures for the earlier part of the tone shows that this average is usually, though not invariably, lower than that for the rest of the tone. Usually the singer begins much lower than the final average and in two or three-tenths of second reaches a pitch much higher than the one to which he finally settles down. This process of sliding up to the tone is much less marked in singing the lower tone and at times it seems as if the process were reversed in these tones, the singer beginning too high and sliding down to the pitch finally adopted. The upper of the two figures in the third column for each tone gives the extreme variation of these earlier tones (the difference between the highest and lowest pitch). Besides this lack of uniformity in the earlier part of the

tone there are other though usually less marked variations in the pitch from any given one-tenth of a second to the next throughout the rest of the tone. The lower of the two figures in the third column for each tone gives the average deviation for the latter part of the tone and is, therefore, a measure of the degree of lack of uniformity of pitch in the main tone.

Turning from these general features of Table II to the individual results, it will be seen that all the subjects show much lack of uniformity both in beginning the tones and in continuing them. Roughly, the amount of this lack of uniformity is proportional to the vibration frequency; the variations in the higher tones being much greater than in the lower. Subjects A., F., and My. are not even approximately correct in singing the low tone, but in all other cases there is a fair degree of approximation to the standard. In these cases it seems clear that the difficulty is to be accounted for in large measure by the inability of the

Table II
Singing before practice.

Subject A.

I			II			III			IV			V		
Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.
Low Tone (S = 100.3 V.D.)														
140			140			133			119			127		
140			149	144.5	9	149	141	16	141	130	22	129		
149	143	9	143			147			142			131	129.0	4
153			149			152			156			128		
146			148			147			149			126		
144			148			148			151			127		
148			148			149			150			129		
146			146			146			150			127		
146			148			148			151			127		
148	147	2.0	145	146.8	1.7	149	148.3	1.3	155	149.3	2.4	129	127.6	1.
High Tone (S = 225.1 V.D.)														
188			180			185			175			172		
202			215			211			224	199.5	49.	215	47	18.
210	200.0	32	218	204.3	38	220	253.3	35	224			219		
213			216			218			220			216		
217			224			215			220			219		
216			220			220			220			216		
215			216			215			220			215		
219			215			220			218			217		
217			215			215			218			218		
217	217.7	1.8	217	217.6	2.5	215			215	219.4	1.8	215	216.6	1.
						218								
						218	217	1.8						

Subject F.

Low Tone (S = 100.3 V.D.)

I			II			III			IV			V		
Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.
143			134			121			150			125		
150	146.5	7	143			139			140			133		
140			150	142.3	16	148	136	27	146	145.3	4	146	134.7	21
144			151			144			146			141		
141			152			146			143			142		
143			143			144			146			143		
141			144			142			143			146		
140			143			145			143			145		
145			150			141			147			145		
143	142.1	1.8	145	146.9	3.6	142	144.9	1.8	143	144.4	1.9	143	143.6	1.5

High Tone (S = 225.1 V.D.)

I			II			III			IV			V		
Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.
187			152			168			171			190		
213			178			201			209			216		
227	209	40	197	175.6	45	219	196.0	51	215	198.3	44	220	208.3	30
216			220			220			212			214		
220			222			218			214			226		
217			220			218			218			216		
219			217			220			218			218		
220			223			217			220			220		
220			215			219			217			220		
219	218.7	1.5	221	221.9	3.0	220	218.9	0.9	219	216.8	2.2	220	219.1	2.7

Subject Ay.

Low Tone (S = 100.3 V.D.)

I			II			III			IV			V		
Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.
112			105			106			116			110		
109			103	104	2	107	106.5	1	107	111.5	9	107	108.5	7
96	105.7	16	109			110			103			113		
117			108			109			105			108		
105			107			104			107			111		
105			108			106			109			110		
102			106			109			103			104		
102			104			106			108			109		
105			106			109			107			106		
108	106.3	3.6	108	107.0	1.3	108	107.6	1.8	107	106.1	1.9	109	108.8	2.1

High Tone (S = 225.1 V.D.)

I			II			III			IV			V		
Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.
179			221			219			236			222		
231			230	225.5	9	228	223.5	9	228			234	228.0	12
219	209.7	40	211			216			217	223.7	19	220		
218			221			227			221			228		
223			221			225			223			221		
226			221			228			219			221		
219			221			223			226			230		
224			230			227			219			225		
221			223			224			223			229		
224	222.1	24	213	221.0	3.6	222	224.0	2.6	218	223.7	2.3	225	224.9	3.1

Subject M.

Low Tone (S = 100.3 V.D.)

I			II			III			IV			V		
Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.
101			98			85			85			109		
99			103	100.5	5	90			101	93	16	112	110.5	3
98	99.3	3	103			100	91.6	15	101			108		
95			106			101			100			108		
101			102			95			99			103		
100			103			100			97			108		
100			101			99			100			108		
103			100			99			99			102		
103			102			101			101			109		
103	102.1	2.2	105	102.8	1.6	102	99.6	1.6	102	99.9	1.2	109	106.9	2.1

High Tone (S = 225.1 V.D.)

I			II			III			IV			V		
Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.
230			221			201			218			225		
222	226	8	227			226			220	219	2	220	222.5	5
225			233	227	12	228	218.3	27	220			220		
221			228			229			222			222		
225			224			221			219			219		
220			225			221			223			223		
228			220			222			221			221		
225			224			221			224			224		
222			221			224			220			220		
226	224	2.3	228	224.3	2.3	220	222.6	2.3	224	221.6	1.6	224	221.6	1.6

Subject R.

Low Tone (S = 100.3 V.D.)

I			II			III			IV			V		
Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.
105			102			107			105			101		
110			102			99			107			102		
115	110.0	10	105	103.0	3	100		8	109	107.0	4	102	101.7	1
112			107			105			110			103		
116			106			107			110			103		
112			106			107			110			101		
115			107			105			109			103		
116			104			107			112			101		
115			102			108			112			104		
112	114.0	1.7	105	105.4	1.4	109	106.0	2.0	110	110.5	1.6	100	102.1	1.3

High Tone (S = 225.1 V.D.)

I			II			III			IV			V		
Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.
177			215			220			186			220		
190			229	222.0	14	229			210			232	228	12
212	193.0	35	224			235	228.0	15	218	204.6	32	230		
216			234			235			222			235		
224			231			231			229			230		
218			235			236			226			232		
220			230			239			230			228		
225			238			237			228			235		
217			234			238			230			232		
220	220	3.0	238	233.0	3.5	240	236.6	2.1	233	228.3	2.5	230	231.5	2.0

Subject My.

Low Tone (S = 100.3 V.D.)

I			II			III			IV			V		
Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.
121			110			118			113			119		
120	120.5	1	112			119			114			116		
121			114	112.0	4	120	119.0	2	118	115.0	5	119	117.5	3
122			114			122			119			118		
121			118			122			120			121		
120			118			122			119			118		
122			119			121			119			120		
120			120			120			120			120		
120			119			122			120			121		
123	121.2	0.9	120	118.3	1.4	120	121.3	0.8	120	119.6	0.5	120	119.6	1.0
High Tone (S = 225.1 V.D.)														
215			215			214			209			221		
225			225			217			215			224	227.5	3
226	220.0	10	225	220.0	10	224	218.3	10	220	214.7	11	222		
226			226			221			217			225		
225			226			223			221			227		
224			226			221			221			223		
228			218			221			219			223		
226			224			223			224			226		
224			222			221			219			222		
220	224.9	1.9	225	224.0	1.8	222	221.7	0.8	221	219.7	1.8	225	224.2	1.6

singer to distinguish the fundamental tone. Indeed, it would appear from the results that A. and F. are perhaps both responding to a prominent overtone.

In carrying out the practice series of experiments subjects A., F., and Ay. were practiced in the singing of the low tone. These practice experiments consisted in the singing of the standard in the manner already described twenty times daily for a period extending over several months. In this way a total number of approximately one thousand tones were sung by each subject. M., R., and My. were practiced in a similar manner with the high tone. When the practice period was about half finished the method was somewhat modified. The subjects were now asked to sing the tone while the standard tone was still sounding. At this point in the experiment and, indeed, from near the beginning of the practice series, all the subjects, except A., were approximating the standards. Singing the tone in unison with the standard resulted in some improvement with the other subjects, but A's. record continued to be very irregular and inaccurate.

In A's. case, therefore, a more radical attempt was made to obtain exactness of reproduction of tone. The tone of the standard fork was now made by striking the fork with a rubber hammer and reinforced by an appropriate resonator. Under these conditions A. responded with tones which were more nearly the tone of the standard, but very inconsistently and with no close approximation of the standard tone, although persistent attempts to obtain correct results were made.

Before obtaining the final results of singing the standard tones after practice, one hundred practice trials were made in the manner originally adopted—that is, with a pause between the standard tone and the beginning of the singing. The results of the final series of five tones at both levels for each of the subjects are shown in Table III.

For more convenient comparison, the results of Tables II and III are condensed in Table IV. Columns V_1 represent the average deviation of the sung tones from the standard before and

Subject A.

TABLE III

Singing after practice.

Low Tone (S = 100.3 V.D.)

I			II			III			IV			V		
Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.
122			118			115			118			113		
120	121.0	2	118			118			120	119.0	2	116	114.5	3
120			113	149.7	5	121	118.0	6	119			113		
118			118			117			119			117		
120			120			119			120			118		
120			116			120			117			113		
119			117			117			118			114		
116			119			121			120			113		
120			118			117			119			112		
118	118.9	1.2	115	117.6	1.3	119	118.5	1.3	118	118.7	0.9	115	114.4	1.3
High Tone (S = 225.1 V.D.)														
153			141			148			144			143		
167			166			182			169			172		
175	165.0	22	173	160.0	32	183	171.0	35	175	162.7	31	176	163.7	33
170			175			179			178			165		
172			178			175			180			170		
174			181			180			177			170		
172			175			178			181			172		
172			176			180			178			170		
172			174			176			180			173		
173	172.1	0.8	178	176.7	2.0	180	178.3	1.7	176	178.6	1.7	174	170.6	2.1

Subject F.

Low Tone (S = 100.3 V.D.)

I			II			III			IV			V		
Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.
98			99			103			104			99		
100			100			103			102			101		
102	100.0	4	103	100.7	4	102	102.7	1	102			103	101.0	4
102			102			103			101			104		
102			103			103			101			102		
101			102			103			101			102		
102			103			103			102			102		
104			103			103			102			103		
104			104			104			102			102		
103	102.6	0.9	102	102.7	0.6	104	103.3	0.4	103	102.0	0.5	103	102.6	0.7
High Tone (S = 225.1 V.D.)														
200			192			210			207			215		
212			219			218			210			220	217.5	5.0
227	213.0	27	221	210.7	29	220	216.0	10	220	212.3	13	220		
222			221			220			222			220		
221			221			221			220			223		
221			215			221			222			222		
221			223			220			222			222		
223			220			221			222			220		
222			225			222			220			221		
222	221.7	0.6	222	222.4	1.6	220	220.7	0.6				223	221.4	1.1

Subject Ay.

Low Tone (S = 100.3 V.D.)

I			II			III			IV			V		
Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.
99			102			101			98			98		
100	99.5	1	103	102.5	1	100	100.5	1	95	96.5	3	99	98.5	1
100			102			101			100			98		
99			102			98			101			98		
100			100			100			101			98		
100			100			100			102			98		
100			98			98			102			99		
101			100			101			101			99		
101			100			101			99			99	98.8	0.7
100	100.1	0.4	101	100.4	1.0	100	99.9	0.9	101	100.9	0.7	101		
High Tone (S = 225.1 V.D.)														
210			191			210			217			215		
216			220	215.5	19	220	215.0	10	220	218.5	3	219	217	4
219	215.0	9	222			218			220			218		
225			220			221			220			220		
220			218			220			220			220		
219			222			220			220			220		
217			220			220	220.0	0.1	219			220		
220			221						221			218		
219			221			220			221			222		
220	220.0	1.4	219	220.0	1.1	221			219	220.0	0.1	219	219.6	1.0
						220								

Subject M.

Low Tone (S = 100.3 V.D.)

I			II			III			IV			V		
Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.
96			99			99			98			95		
100	98.0	4	105	102.0	6	101	100.0	2	100	99.0	2	103	99.0	8
92			100			98			99			101		
94			100			102			100			100		
95			100			102			101			100		
96			97			100			103			101		
99			99			102			102			101		
99			98			103			103			100		
97			101			102			103			102		
94	95.8	2.0	99	99.3	1.0	102	101.4	1.2	101	101.5	1.3	101	100.8	0.6
High Tone (S = 225.1 V.D.)														
210			220			210			220			212		
218			220			221			230	225	8	223	217.5	11
226	214.7	16	224	221.3	4	222	217.7	12	219			223		
227			225			223			225			223		
229			225			222			224			222		
229			225			224			224			223		
227			226			224			223			224		
228			225			225			223			224		
227			225			225			225			223		
225	227.4	1.1	225	225.1	0.2	227	224.3	1.2	225	223.5	1.4	224	223.3	0.7

Subject R.

Low Tone (S = 100.3 V.D.)

I			II			III			IV			V		
Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.
105			107			107			105			109		
106	105.5	1	103	105.0	4	107			107	106.0	2	106	107.5	3
104			108			108	107.3	1	106			108		
105			109			107			107			109		
102			106			106			107			107		
103			108			105			108			109		
107			107			106			107			109		
102			109			107			108			108		
102			105			105			108			109		
102	103.4	1.5	105	107.1	1.4	105	105.9	0.7	106	107.0	0.7	109	108.5	0.6
High Tone (S = 225.1 V.D.)														
220			215			216			219			219		
223			220			225			223			222		
225	222.7	5	221	218.7	6	228	223.0	12	225	222.3	6	224	221.7	5
228			225			228			223			222		
225			225			229			225			224		
228			225			230			224			224		
225			226			229			224			224		
228			225			229			226			226		
228			227			230			226			223		
229	227.3	1.3	228	225.7	1.0	228	229.0	0.1	228	225.0	1.3	225	224.0	0.1

Low Tone (S = 100.3 V.D.)

Subject My.

I			II			III			IV			V		
Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.	Pitch	Av.	Dev.
141			141			137			148			140		
143	142	2	144	142.5	3	145	141	8	143			138		
140			139			137			138	143.0	10	145	139.0	2
150			148			142			142			148		
146			139			142			146			142		
142			143			143			141			145		
143			140			143			146			145		
147			146			142			145			140		
143			142			138			142			143		
143	144.3	2.1	142	142.6	2.8	145	141.5	2.0	143	143 6	1.8	142	143.8	2.1
High Tone (S = 225.1 V.D.)														
231			228			230			229			218		
239	235.0	8	228			222			227			213	215.5	5
237			225	227.0	3	228	226.0	8	224	226.7	5	220		
232			226			223			227			213		
231			231			226			221			222		
229			230			224			230			211		
224			225			227			230			221		
228			228			223			223			216		
230			224			220			229			218		
228	229.9	2.6	227	227.3	2.1	229	225.1	2.4	222	226.0	3.4	212	216.6	3.6

TABLE IV

Subj		Low			High		
		V_1	V_2	V_3	V_1	V_2	V_3
A	Before	43.5	1.7	1.2	7.4	1.8	40.2
	After	17.3	1.2	3.6	49.8	1.7	30.6
F	Before	44.0	2.1	15	6.0	2.1	42.0
	After	2.3	0.6	3	3.7	1.0	17.0
Ay	Before	6.9	2.1	7	2.0	2.8	17.8
	After	0.6	0.8	1.4	5.2	0.8	9.0
M	Before	2.4	1.8	8.4	2.3	2.2	10.8
	After	1.7	1.2	4.4	1.3	0.9	10.2
R	Before	7.3	1.6	5.2	6.8	2.6	21.6
	After	6.1	1.0	2.2	1.6	0.8	6.8
My	Before	19.7	0.9	3.0	2.2	1.6	8.8
	After	6.9	2.2	5.0	3.3	2.8	5.8

after practice for both high and low tones. In arriving at these averages the earlier two or three-tenths of a second of the tone were not taken into account since, as we have already seen, they are not representative of the tone as a whole, but of a period of initial adjustment. Columns V_2 give the average of the mean variations of the five tones for each subject before and after practice. These figures, therefore, represent the degree of the

steadiness of the tone as a whole, disregarding as before the initial period. In columns V_3 are arranged the average of the variations at this initial period for the five high and five low tones before and after practice. The figures of V_1 show that A., F., and Ay., who practiced with the low tone, reduced the error in singing at that level. In the case of A., however, the improvement is more apparent than real, for at no time does he approximate the standard. His records from day to day show no gradual improvement as in the case of the other subjects and no constancy of results. The introspective records show that this subject toward the end of the practice series responded in a hit-or-miss fashion with a tone near the lower level of his voice register. When this subject began his practice on any given day with a sung tone of a certain pitch, his subsequent tones were usually of about the same pitch. This tendency to persist in repeating a tone of the same pitch when once begun was found to be characteristic of non-musical subjects in my former study of singing reactions (3) and also in the work of Miles (7). It would appear that such persons in the absence of complete organization of the auditory and vocal motor factors rely mainly upon a memory of the kinaesthetic sensations as cue to further reproductions of the tone.

In the case of F. who began with an error as great as that of A. and in the same direction, a tone was sung on the second day of practice which approximated the standard and improvement thereafter was of a steady character, quite in contrast with the erratic nature of A.'s record. Ay. began with a tone approximating the standard and made steady improvement.

M., R., and My. were practiced in the singing of the high tone. M. and R. made consistent improvement in the amount of error. The figures given for My. seem to indicate a lack of capacity for training with the high tone of a similar nature to that found in the case of A. with the low tone. The similarity, however, does not hold in one respect. My.'s results were, indeed, erratic during the course of practice but he frequently approximated the standard to a much higher degree than did A. This was especially true of that part of the practice series during which the

singing took place simultaneously with the sounding of the standard fork. Indeed, it was true of all the subjects except A. that a marked improvement took place during this part of the practice series. In the case of My., however, this improvement was not carried over when the original conditions were restored, while in the other cases it was.

At the end of the practice series, then, F., Ay., M. and R. had made improvement in the singing of tones of the pitch on which they had practiced. At this stage the average errors, respectively, for these subjects amounted to 2.3%, 0.6%, 0.6%, 0.7% of the standards.

Turning now to a consideration of the effects of the practice on the singing of tones at a different level, it will be seen that of those who improved in the practice series F., M. and R. made a slightly smaller amount of error in these non-practiced tones after practice than before practice, while Ay. did worse. There is, therefore, little evidence of transference of practice effects in the accuracy of singing from one pitch to another.

The direction of the error represented by the figures in column V_1 is not indicated in the table, these figures being the arithmetical average of errors. A detailed study of this point shows, however, that there is a general tendency to sing the lower tone too high, and the higher tone too low. This statement is unequivocally true of the tones sung by A., F., M. and Ay., both before and after practice. In the case of R. both the low tone and the high were sung too high before and after the practice series. My. always sang the low tone too high but sang the high tone too low before practice and too high after practice. Miles (7) found a general tendency to sharp in male subjects but the fact that his standard tones covered a narrower range within easy compass of his subjects' voices made his results scarcely comparable with our own.

Seashore and Jenner (9) report improvement in accuracy of singing tones after practice with visual control by means of the Seashore tonoscope. In these experiments the improvement apparently transfers to tones of a different pitch from those practiced. That there should be more evidence of transfer under

such conditions where the visual control gives a knowledge of results might be expected from other experiments in transfer (4), (6), (8).

A comparison of columns V_2 and V_3 before and after practice shows improvement in approximation to uniformity of tone both for the tones which were practiced and those which were not in almost all cases. The only exception is in the case of My., all of whose tones were less uniform after practice than before. The improvement with respect to uniformity is not marked in the case of A. and also in the case of M.'s high tones so far as the extreme variations at the beginning of the tone are concerned (V_3).

Berlage (1) has found that the deviations from period to period are less when the reproduced tone is sung in response to one's own sung tone than when the standard tone is the sung tone of another person. He also found that the deviations are greater when the tone is sung freely without any attempt to approximate a standard than it is under either of the conditions just mentioned. It seems, therefore, that the steadiness with which a tone is maintained is not a purely physiological phenomenon of muscular tetanus but that it varies with the psychological conditions and is subject to improvement through practice. Furthermore, the effects of practice, so far as steadiness is concerned, are transferred to tones of a different pitch from those practiced, as my present investigation shows. The same statement applies to the larger deviations at the beginning of the tone.

It remains to give the results of the tests for discrimination after the practice series of experiments. These tests were given in the same way as those made before practice. The results are given in Table V and the average results before practice are added for purposes of comparison.

This table shows that F., Ay., M. and R. have lower thresholds of discrimination at the level at which practice in singing took place than was the case before practice. This improvement is, however, not extended to the other non-practiced level. It will be remembered that these subjects all made unequivocal improvement in the amount of error made in singing their standard

Table V
Discrimination-Thresholds after practice

	Low						High					
	A.	F.	Ay.	M.	R.	My.	A.	F.	Ay.	M.	R.	My.
1.	2.4	.9	1.3	1.9	1.3	2.4	3.8	2.0	2.0	1.1	1.1	2.0
2.	2.4	1.3	1.3	1.9	1.3	1.9	3.8	2.6	2.0	0.5	1.5	2.6
3.	2.4	0.9	1.3	1.3	1.3	1.9	2.6	2.0	2.0	0.5	1.5	2.6
4.	1.9	0.9	1.3	1.9	0.9	1.9	3.2	2.6	2.0	0.5	1.5	2.0
5.	2.4	1.3	1.3	1.9	1.3	1.9	3.2	2.6	2.0	0.5	1.5	2.0
Av.	2.3	1.1	1.3	1.8	1.2	2.0	3.3	2.4	2.0	0.6	1.4	2.2
Before Practice	2.2	2.2	2.2	1.4	1.3	1.8	3.4	2.6	2.1	1.8	2.6	2.1

tones. In the case of A. and My. no improvement is shown in the pitch discrimination after practice in singing. As has already been shown these subjects made no real progress in the reduction through practice of the error in singing the standard tone.

Comparing tables IV and V it is found that in general the figures expressing sensory discrimination are lower than the corresponding figures for the sung tones. This is true in all but three of the twenty-four pairs of results. In the three cases where this is not true, the figures are practically the same. It would, therefore, appear that fineness of discrimination is more accurate than motor reproduction. This point becomes very marked when it is remembered that the figures given for sensory discrimination are probably much higher than would have been obtained from tests of sensory discrimination given under the more standardized threshold conditions.

An inspection of the figures would lead us to expect little correlation between sensory discrimination and accuracy of reproduction for the same tone. Calculated by the Pearson products-moment method the index of correlation is .37 with a probable error of .023. Miles (7) found the index of correlation between accuracy of singing and pitch discrimination for eighty-two male subjects to be only .33. The most marked lack of correlation between accuracy of reproduction and sensory discrimination is in the cases of A. and F., the subjects who made little or no improvement in accuracy of singing but who nevertheless are not noticeably different from the others in the results of the discrimination tests. That practice results in a lowering of the threshold of pitch discrimination has been reported by

Seashore (**10**) and also Smith (**11**). Seashore distinguishes a so-called "cognitive" threshold from a "physiological" threshold and holds that the cognitive threshold only is subject to improvement through practice. It is clear, however, that the physiological threshold is a somewhat theoretical limit. It must be admitted, however, that the threshold found in this investigation is a "cognitive" threshold found under special conditions. The subjects were required to be certain not only of a difference in pitch but of the direction of the change. While this kind of cognitive threshold, therefore, was peculiarly subject to improvement by practice, it seems nevertheless true that the improvement which actually took place was due to the practice in singing rather than to adaptation, attention, interest, or other factors. Such factors would presumably be operative to as great a degree at the non-practiced level as at the level at which practice took place but, as we have seen, the improvement was not transferred from one level to another, either in the singing or sensory discrimination.

The results of the present study tend to confirm the objections that have been made to those so-called motor theories which regard kinaesthetic sensations as playing the fundamental rôle in such responses. The results show that motor reactions to tones are by no means so accurate as the sensory discrimination. Judd's study of the eye-movements in the perception of the Müller-Lyer illusion (5) gave analogous results. He found that there was an intimate relationship between the character of the eye movements and the amounts of illusion, and also that both the eye-movements and the amount of illusion changed with practice. Nevertheless, the movements are of such a nature as to preclude the probability that the resultant kinaesthetic sensations are the cause of the illusion.

On the other hand, there is agreement between these results and those of other previous investigations which have shown the closeness of the relationship between sensory and motor processes—a relationship involving motor organization rather than mere kinaesthetic sensations. In my earlier investigation it was shown that the motor reactions to tones are

intimately related to all the forms of harmony and discord. Bingham's investigations (2) have led him to the conclusion that melody is related to the "upsetting of established muscular tensions," "the organization of incipient responses," and the merging of balanced tensions. Stetson (12) has found similar tension-relaxation processes in his experimental study of rhythm. The facts here presented point to the organic unity of motor and sensory factors in even so relatively simple a process as sensory discrimination of tones. With the development of more precise and unvarying modes of response to one tone, there arises a greater keenness in discriminating that tone from all others. Sensory discrimination must, therefore, be regarded as related to the organization which has taken place with reference to the new mode of response.

SUMMARY OF RESULTS

(1) There is no marked correlation between the initial capacities of the subjects tested for discrimination of tones and ability to reproduce these tones accurately by singing. A subject (A) whose "ear" is little inferior to that of the other subjects is nevertheless totally at a loss to sing the tones accurately.

(2) Practice in singing tones of a certain pitch resulted in marked reduction in the error of reproducing those tones in the case of four of the six subjects.

(3) Slight improvement in singing tones of a pitch different from the one practiced was made by three of those four subjects and no improvement by the other.

(4) Practice resulted in improvement in steadiness both at the initial point of the sung tone and throughout the tone as a whole in the case of those subjects who improved in accuracy.

(5) Improvement in steadiness was also shown in the singing at the non-practiced level.

(6) Subjects who improved in accuracy of singing tones of a certain pitch improved also in discrimination of tones at that level.

(7) There was no improvement for such subjects in the discrimination of tones of a different pitch from that practiced.

(8) Subjects who did not improve in accuracy of singing made no improvement in discrimination.

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AN EXPERIMENTAL STUDY OF THE CONSCIOUS ATTITUDES OF CERTAINTY AND UNCERTAINTY

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The object of the present investigation is an analysis of the conscious attitude (*Bewusstseinslage*) on the motor side. The effort is made to determine whether certain regular forms of bodily reaction accompany the subjective attitude of certainty, and whether with changes in subjective attitude there are corresponding changes in the forms of such reaction. The subject is given a task which he carries through in response to certain instructions. The motor processes immediately involved in carrying through this reaction to the instructions are recorded by the apparatus, and at the end of each experiment the subject gives a complete introspective report. These two records, the objective record of the form of the reaction and the introspective record of the conscious processes experienced during the reaction, are examined together to see if there is any correspondence between them. The tasks chosen for the investigation consist in the making of drawing movements with the hand out of sight. The apparatus gives records of the accuracy of these movements, and of all changes in rate and pressure which occur during the process. Variations are introduced to determine whether by changing the instructions in such a way as to modify the objective form of the reaction a corresponding change may be produced in the subjective attitude.

The conscious attitude has come to hold a very important place in psychological theory, and a good deal of work has been directed toward analyzing it by the introspective method. Yet, to the writer's knowledge, no attempt has been made to attack it on the motor side. The present investigation attempts this, its object being to find out whether the introspectively given attitude of certainty involves a definite form of motor reaction on the part of the subject, and a change in the attitude a change in the form of such reaction. The method used is an extension of that which has already been employed in the investigation of the relation between motor processes and consciousness. Judd (2) has shown that the character of perception and attention is closely connected with motor processes. The present work follows the

same method, extending it to that phase of consciousness which experimenters in the field of the higher thought processes have called the *"Bewusstseinslage"* or conscious attitude. This investigation is, therefore, more closely related to the experimental work on the relation between movement and consciousness than it is to the almost exclusively introspective experimental investigations of the higher thought processes.

The first requisite in an investigation of this sort is a motor process, simple enough to be measured objectively, and yet sufficiently difficult of performance to yield the necessary richness of introspective data. The making of simple drawing movements with the hand hidden from sight was chosen as the task. These drawing movements were measured by the apparatus, and after each drawing full introspections were given by the subject and taken down by the experimenter. The drawings were made in response to certain instructions, and in the course of carrying out the process various attitudes appeared, as reported in the introspections. Now if there is any relation between attitude and motor processes in general the attitudes reported here must be connected with the particular motor processes measured by the apparatus, because both attitude and the processes measured come of necessity in response to the instructions. In other words, the motor processes, i. e., the drawing movements, measured by our apparatus are necessarily involved in carrying out the instructions, and are therefore particularly relevant to any attitudes consequent upon the instructions.

The apparatus for measuring the drawing movements was that used by Freeman in his analysis of writing movements (1). The two cuts of the apparatus which appear there are reprinted here (Figs. 1 and 2).

This apparatus yields three objective records of the drawing movements. In the first place there is the actual drawing made by the subject upon the primary sheet. The task was usually the copying of a simple geometrical figure—the "model." Thus when the drawing is measured for straightness of lines, length of lines, size of angles, etc., and these measurements compared with the corresponding measurements of the model, a record of

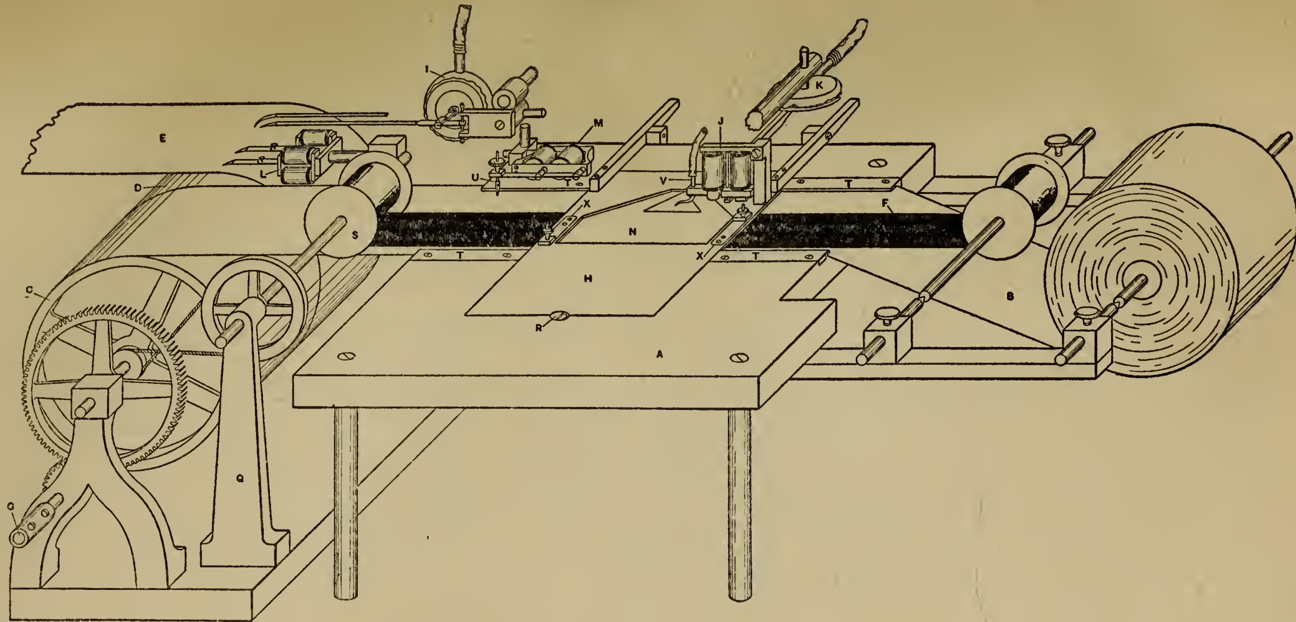


FIG. 1.

The primary sheet on which the reactor draws is fixed under the hinged plate, H (see also Fig. 2), and is exposed through the opening of the plate, N, to form the surface on which the drawings are made. Below this sheet runs the moving strip, B, for the time record, and between sheet and strip runs the type-writer ribbon, F, at a much slower rate. The original drawing and the secondary drawing made through the type-writer ribbon on the moving strip, are correlated through the two pencil-points, XX, which project through two holes in the plate, H, and make two dots on the primary sheet and two lines on the moving strip. The first time-marker, J, writes through a hole in the primary sheet directly upon the moving strip. Below the opening, N, is the hingedplate, C (Fig. 2), and this plays upon the lever which is connected with the tambour, K. In our arrangement the tambours, K and I, were removed, and another pressure-recording device substituted. (See text.) E is the smoked strip for the pressure record, and L the second time-marker, in circuit with the first. D is the drum for the pressure record, and C the drum which unrolls the moving strip. The marker, M, not needed in these experiments, was removed.

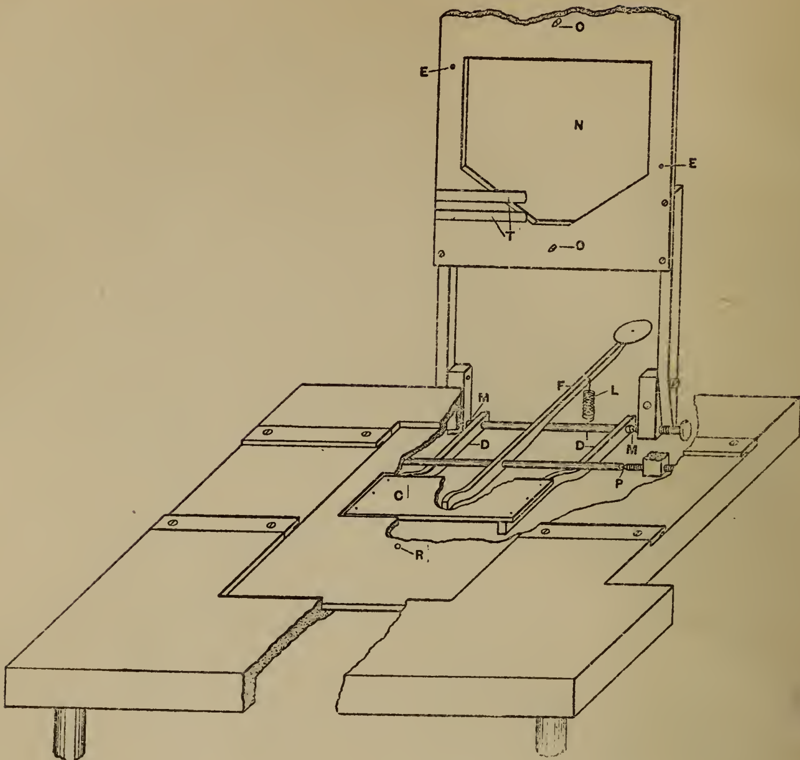


Fig. 2.

accuracy is obtained. In the second place, the apparatus gives a time record for the whole drawing or any part of it. As the subject draws upon the primary sheet his drawing is registered through the typewriter ribbon upon the moving strip. This gives a record of the whole drawing spread out under a standard time-line traced by the first marker. By correlating this with the original drawing in the manner described by Freeman, it is possible to follow the details of time changes throughout. In the third place, the apparatus gives a record of pressure. The drawings are made above the hinged plate which sinks slightly when pressure is exerted upon it. This movement of the plate, much magnified, is communicated to a pointer which traces a curve upon a smoked strip. In this way all changes in the pressure exerted by the subject in drawing are registered. This pressure

record is correlated with the records of time and accuracy through a second standard time-line traced upon the smoked strip by a second marker in circuit with the other. The time is given by a Kronecker Interrupter located in another room, marking tenths of a second. Three records are, therefore, obtained from each drawing movement, showing its accuracy, the rate of drawing throughout, and synchronous changes in pressure.

Several changes were, however, made in Freeman's apparatus. A finely-pointed lead-pencil was used in place of his capillary pen for the time-record on the moving strip. An electric motor was used to drive the apparatus. This motor was encased in a wooden box heavily lined with felt to shut in the noise. Its speed was reduced through a worm-gear mounted in a standard on the top of the box. The drum, C, Fig. 1, was turned by a string-belt from the driving-wheel of the worm-gear. This method lacks only one of the advantages of the original arrangement. It is not now possible to start the apparatus at full speed, as could be done when the friction clutch was used. This proved to be no detriment, however, as it took the apparatus only a very short time—about two seconds—to develop its maximum speed. This time was just about the interval the experimenter allowed between the starting of the motor and the giving of the signal to begin drawing. The chief advantage in using the motor is that it eliminates a great deal of distracting noise.

A third and more important change in the original apparatus was the substitution of a new pressure-recording device for the tambours. The tambours were used in the first practice series, and were found unsatisfactory for the purposes of this experiment. The pressure in a drawing movement is usually less in amount and more gradual in its changes than that in a writing movement. This calls for a recording device which is more sensitive and which magnifies the record to a greater degree. There is always a stretch to the rubber of the tambours and to the walls of the connecting-tube. The movements are thus softened down so that some of their characteristics are lost. The simple device used in these experiments does away with this deficiency, as well as satisfying the other conditions.

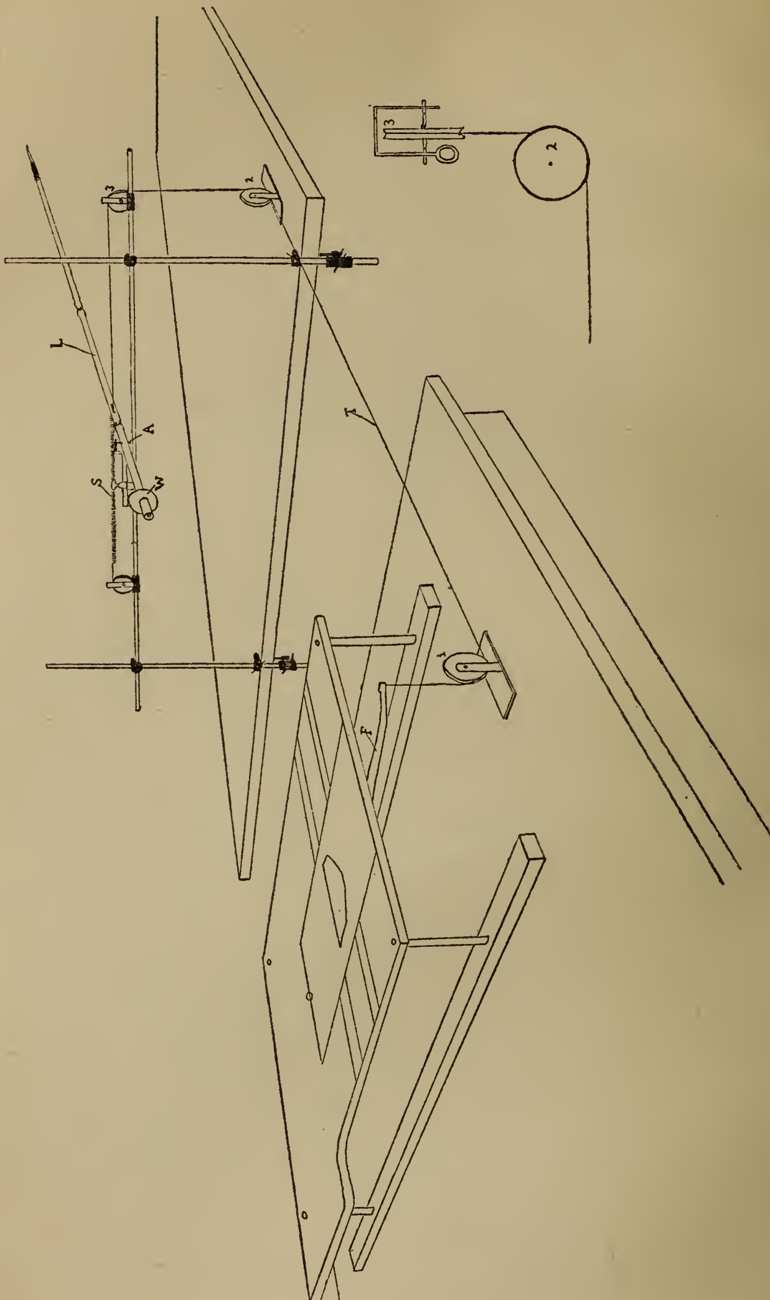


Fig. 3a.

Fig. 3.

The two tambours, I and K, Fig. 1, were removed, as was also the metal disc on the end of the lever, F, Fig. 2. A new recording-pointer was made. As this pointer was of considerable length, the drum D, Fig. 1, over which the smoked strip runs, had to be moved away from the rest of the apparatus. It was accordingly placed about a meter away from the drum, C, and run by a string-belt from a wheel on the axle of the drum C, to a wheel of the same size on its own axle. The marker, L, Fig. 1, was held by a special standard which stood on the table. The new recording-pointer was made of a thin piece of bamboo, L, Fig. 3, 60 cm. in length. At the recording end it was supplied with a metal recording-point. Near the other end it was pierced by an axis, A, which was then mounted in bearings. The movements of the lever, F, Fig. 2, were communicated to the recording-pointer by means of a thread running over a system of wheels. These wheels were made of wood and were supplied with fixed axles mounted on bearings, friction being thus reduced to a minimum. Fig. 3 shows how the movements of the lever, F, are communicated to the recording-pointer, L, which is at right angles to it and on a different level. The wheels over which the thread, T, runs are numbered 1, 2, and 3. An end view of the arrangement of wheels 2 and 3 is given in Fig. 3a. The thread was attached to the recording-pointer between the axis and the point—9 cm. from the axis and 42 cm. from the point. At this place too, on the other side of the pointer, there was attached a long fine spring, S, the tension of which could be readily and delicately regulated. A small round weight, W, Fig. 3, with a circular hole in the middle, was placed over the short end of the recording-pointer to make its weight equal on either side of the axis.

Another addition to the apparatus was the screen for cutting off the subject's view of his hand as he drew. The screen was of grey card-board, held in place by means of clamps, which were in turn held by uprights clamped to the large surface, A, Fig. 1. Another large screen of black card-board was used to cut off the subject's view of the pressure apparatus.

The marker, M, Fig. 1, used by Freeman to record the giving

of the signal in reaction experiments was not needed for our purposes, and was removed. A slight change was made in the arrangement of the marker, J. Instead of its being fastened to the plate, H, it was held just above it in a clamp attached to a rod running from one of the uprights holding the screen. To this same upright, higher up, were also attached the rod and clamp for holding the card on which the model was drawn. The models were drawn in black on white cards 12.5 cm. x 7.5 cm. The figures used are illustrated in Fig. 4, and will be described in detail in the statements of the individual series. The method of measurement adopted in each case will also be described there.

The apparatus was run more slowly than in Freeman's experiments, drawing movements being made more slowly and with more gradual changes in speed and pressure than writing movements. Moreover, the experiment is of such a character that it does not demand as minute an analysis of the movements as did Freeman's. Accordingly the apparatus was run just fast enough so that the records could be read in 100ths of a second.

In an investigation of this sort another requisite is that the subjects give full introspections. In our experiments they were instructed to tell after each experiment everything that had gone on in consciousness from the giving of the signal to begin the drawing to the end of the drawing. The introspective reports were not confined to this period, however, and general remarks made by the subject were also accepted by the experimenter though they were not given the same weight as the introspective report. Sometimes the experimenter increased the fulness of the report by appropriate questions, but care was taken that these should not be suggestive. Those who served as subjects were all trained psychologists. They were Professors Angier (A.) and Cameron (C.), Dr. E. P. Frost (F.), Instructor in Psychology, and Mr. S. L. Reed (R.), a graduate student in Psychology. To these men I am indebted not only for the large amount of time they have given me as subjects, but also for many pieces of helpful criticism and advice. The subjects were kept as naïve as possible. With the exception of A., none of them knew the purpose of the experiment, or anything about the results. A.

knew the purpose of the experiment, having himself suggested the problem, but he entered upon the investigation unconscious of any bias, and without anticipating any of the results. He was also kept in ignorance of his own results, though he did examine some of the results obtained from other subjects in cases where their task had been different from his own.

The method of procedure was as follows. Before each experiment the model was placed in its holder. The subject seated himself before the apparatus, took the pencil, and placed his hand under the screen ready to draw. He then received his instructions. The experimenter, after a preliminary "Ready" signal, started the motor and the two time-markers and, finally, gave the signal "Now" for the subject to begin drawing. This succession—"Ready," starting of apparatus, and "Now"—was not carried through in connection with any time-regulating mechanism, but the intervals were kept as nearly equal and constant as possible. As soon as the drawing was finished, the experimenter stopped the apparatus and took down the subject's introspections.

The instructions in these experiments, though not given in a stereotyped formula each time, were nevertheless definite and as brief as possible. At the beginning of a series the experimenter described to the subject in exact terms how the figure was to be drawn, i. e., that he was to begin at a certain definite point, proceed in a certain direction, and make his drawing as nearly like the model in size and form as he could. These instructions were briefly repeated for each drawing of the series. In all the earlier experiments no instructions were given concerning the rate of drawing or the pressure to be exerted, since variations in these objective factors, rate and pressure, were to be correlated with variations in introspective attitude, to give instructions about them would be to predetermine one of the variables. Later on, however, special instructions as to rate, and in a few cases as to pressure, were given in order to see if by purposely varying these objective factors a corresponding variation in attitude could be obtained, such as preceding experiments might have led one to expect would occur. When such special instructions were given they were always repeated in exactly the same words before each drawing.

At the start the subjects were told to report in their introspections all conscious processes that accompanied the drawing of the figures. No hint was given as to what processes were the object of the investigation, and all attitudes reported were given equal consideration. Early in the investigation, however, it was found, as had been to some extent anticipated, that the only attitudes that appeared universally enough to admit of systematic study and correlation were attitudes designated as "certainty" and "uncertainty." It was therefore to these attitudes that the experimenter directed his attention, and the variations introduced into the experiment were aimed directly at bringing them more clearly into the light. Moreover, the questions by which the experimenter enlarged the subject's introspective report often had to do directly with the attitude of certainty, although they did not reveal to the subject the fact that this attitude was the object of the investigation. The subjects still continued to report all the other attitudes, images and sensations they had experienced. It may be that other observers would distinguish other attitudes within the certainty and uncertainty experienced in a drawing process. However this may be, our subjects reported definite conscious attitudes, some of which were designated as attitudes of certainty and others as the opposite attitudes of uncertainty, and the contrast between them holds whether or not they might be called by other names or each divided into a number of different sub-classes. Certainty, however, may be of two kinds, as reported by our subjects. These are designated as positive certainty and negative certainty. Positive certainty is the attitude that the drawing made is correct. Negative certainty is the attitude that the drawing made is incorrect in some definite way. Uncertainty is the attitude that the subject can't tell whether the drawing approximates the model or not. The distinction between positive and negative certainty involves a distinction between the attitude of certainty and the accuracy with which the subject feels the drawing to have been made. The attitude of certainty might be that the drawing was accurate or it might be that it was inaccurate. In either case the attitude was certainty.

Certainty in a drawing may give way to uncertainty and vice

versa. Sometimes the attitude refers to one portion of the drawing, sometimes to the drawing as a whole. In the longer drawings there is reported a continuous attitude, which lasts throughout, varying between certainty and uncertainty. In shorter drawings the attitude may appear just as the subject finishes, and in such cases it usually refers to the drawing as a whole. There are, moreover, different grades to the attitude. At times certainty or uncertainty is more intense than at other times. These differences are reported by the subject in such terms as "good certainty," "only fair certainty," "a little uncertain," "typical uncertainty," "absolutely uncertain," etc.

In order to show a correlation between the attitudes reported and the objective records, it is evident that a fairly large number of experiments must be made, for the safety of this correlation depends upon the number of experiments and the taking of careful introspections. With the more complicated models that were used first in our experiments, twenty drawings were made of each model. With the simpler drawings, ten experiments were usually obtained for a series. In all about six hundred and fifty experiments were made.

We turn now to a consideration of the individual series of experiments.

Series I. Practice Experiments

The first experiments were conducted in the spring of 1912, chiefly for the purpose of finding out whether the apparatus and method were adequate. As already stated, these preliminary experiments showed that a new method of recording pressure would have to be devised and substituted for the tambours.

The drawing process in this series was the same for all subjects, the model being a single straight, vertical line, 27 mm. in length (Fig. 4, No. 1). The task was to draw three vertical lines, equal in length to the model line, parallel to each other, and equal distances apart. They were to be drawn from top to bottom, and the whole process to progress from left to right. At the close of the drawing the subject gave his introspections, and named the order of certainty of the lines he had drawn. He

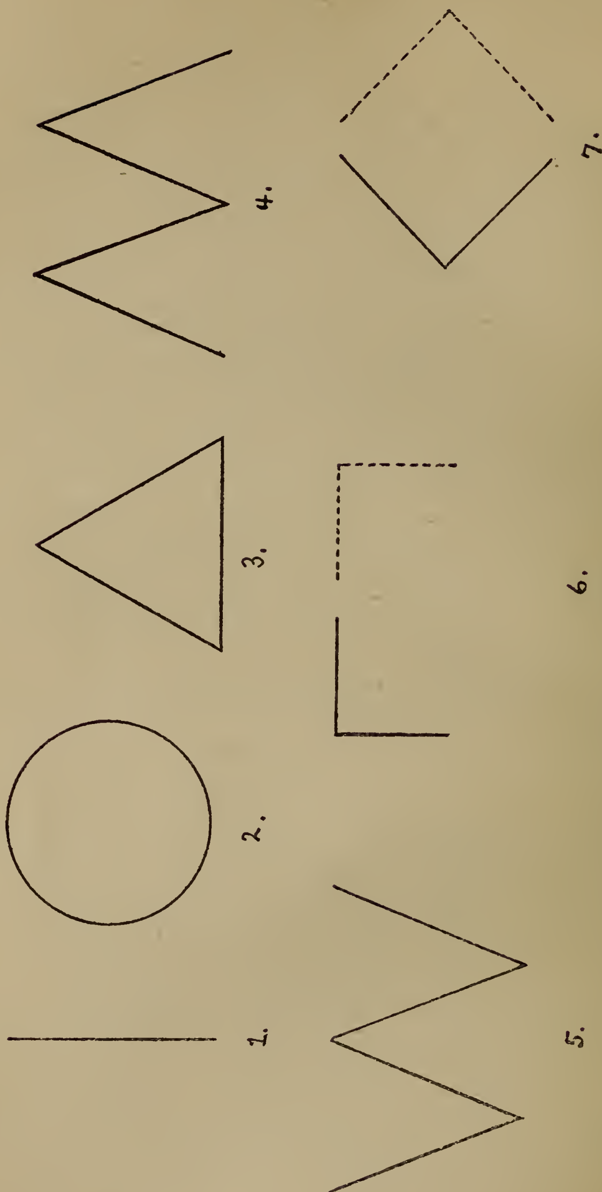


Fig. 4

ranked them from most certain to least certain. After a few experiments had been made, it was found that the first of the three drawn lines came to be regarded, without intention, as a sort of preliminary affair in which the subject simply "got the swing" of the process. The result was that he had little introspective knowledge of it, and it could not be compared with the other two lines. Since the introspective comparison of three lines was desired, this difficulty was obviated by having the subject draw four, the first of these to be a preliminary one to get the process started.

The lines in the drawing made by the subject were measured for accuracy as follows. *First,* their length was measured in millimeters. *Second,* an estimate of the straightness of each line was made. This was expressed in two ways. First the number of little waves or turns in direction was noted, and second the amount of deviation of the whole line from the vertical—determined with reference to a perpendicular to the horizontal boundary of the bottom of the space in which the subject drew. The distance from this perpendicular of the top of the line was measured and compared with a similar determination of the bottom of the line. Their difference, expressed in millimeters, gives the second measure of straightness. *Third,* the width of each interspace was calculated. This was done by taking the average distance apart of the two lines bounding it.

For purposes of time measurement the lines were divided into three equal parts and the time for each third calculated. In nearly every line, moreover, there were two periods, one at the beginning and another at the end, during which the pencil was touching the paper but not moving. The subject was not aware of the fact that he did not begin to draw the instant his pencil touched the paper, nor did he know that at the end of the line he held it still for a fraction of a second before raising it. The time for these two periods was also determined, making in all five time determinations for each line. Finally, the time between the end of each line and the beginning of the next, or the time in the interspaces, was also calculated.

These practice results are incomplete, the pressure records

being defective, and are hardly significant except as they point toward later results. The records of the lines which each subject called most certain, and those for his least certain lines were grouped together and averaged. In the cases of A. and R. the average most certain line is drawn at a slightly faster rate than the average least certain line. In the case of C. the opposite is true here, but later results do not bear this out. With F. it was impossible to compare the most certain and least certain lines, because of lack of complete introspection.

Certain differences in imagery between the individual subjects came out in the practice series, and, as they keep appearing throughout the whole investigation, it will be well to describe them here.

A.'s imagery is almost entirely visual. In starting a drawing he has a visual scheme of it to be filled in. This visual scheme is followed throughout, and any marked deviation from it is pictured in visual terms. Sometimes the visual scheme is weak and the imagery vague. In making a drawing without a model it seems important for this subject to keep his eyes fixated upon the cardboard screen in the general direction of his hand. In drawing from a model he keeps his eyes upon the card, but his tendency is, as he expresses it, "to use the model as a suggestion rather than as a plan," that is, to draw rather from his own visual image of the figure than from the figure itself.

C. reports chiefly kinaesthetic imagery and sensations in the drawing process. Any plan or scheme he has at the beginning of a drawing is in kinaesthetic terms. He sometimes prepares for a drawing by making incipient movements with the pencil. He is easily disturbed by any slight awkwardness in the manner of holding the pencil, or in the position of the hand. In drawing from a model he follows it with his eyes throughout. In drawing without a model he does not keep his eyes fixated in a particular way, but gets his hand placed carefully and then looks away. He reports visual imagery too, but his visual image always comes up after the drawing has been made, as the last stage of the process.

F. reports imagery of both types. The most prominent kind

of imagery with him is visual-motor. He images the developing figure as he draws, and he often visualizes the moving pencil-point as well.

R. reports less imagery than any of the other subjects. What he does report is chiefly visual, with occasional kinaesthetic, imagery. Often incomplete visual images appear. Part of a line will be visualized and the rest will not appear. Sometimes a line will be visualized as heavy and dark at one end, but gradually shading off into nothing at all at the other end.

Series II, IIa and IIb

The remainder of the experiments were made in the fall of 1912, and the winter and spring of 1912-13. In the present series each subject was given a different figure to draw, and so a different task. Series IIa is in each case a variation upon Series II. In treating these series we will take up the different subjects separately.

Subject A.—A. was given as a model a circle, 27 mm. in diameter (Fig. 4, No. 2). He was instructed simply to copy the circle exactly, beginning at the top and drawing in the anti-clockwise direction, using a finger movement and not shifting the position of his hand during the drawing. The anti-clockwise direction was chosen because it seemed to the subject that it would be more natural for him than the clockwise direction. After taking a series of twenty such anti-clockwise drawings a variation was introduced by having the subject draw the circle in the opposite direction, still beginning at the top. A series of twenty such clockwise drawings was also obtained, forming Series IIa. Then, after a long interval (October to April), during which a large number of other experiments had intervened, another series of ten anti-clockwise drawings was made. These form Series IIb.

The method of measurement for these drawings was as follows. For accuracy, the approximate center of the circle drawn by the subject was found with a compass. Then four axes were drawn through it, the vertical, the horizontal, and the two diagonal axes. When compared with the diameter of the original

circle, the lengths of these axes give a good estimate of the accuracy of the circle as to size. When compared with each other they give a good measure of the roundness of the circle. In measuring the accuracy of the circle, also, the distance from end to end, that is, the amount by which the end point failed to meet the starting-point, was measured in millimeters. This measurement is expressed as the "distance from end to end" under the abbreviation "E. to E."

For measuring time changes during the process of drawing, the circumference was divided into quarters. A more minute division of the circumference seemed unnecessary because of the character of the subject's introspections. These were nearly always given in terms of halves or quarters of the circle, the subject making these divisions guiding marks in his description of the course of mental processes during the drawing. It was therefore evident that any changes in drawing rate which could be correlated with a change of attitude should show in a comparison of the quarters. The end-points of the vertical and horizontal axes were taken as marking the boundaries of the quarters of the circumference. This, of course, is not absolutely accurate as the drawn circle was never perfectly round, but it is exact enough for our purpose. The pressure record was also divided into four quarters.

For brevity and convenience in exposition, the results of these series will be treated as a whole by a consideration of the averaged measurements of the twenty drawings and a summary of the introspection. The individual drawings show such uniformity in their characteristics that this may be done.

The introspections for both Series II and IIa show that a continuous attitude accompanied the drawing of the circle from start to finish. The anti-clockwise circles are more certain than the clockwise. This is shown both in the relative number of attitudes of certainty reported, and in the general remarks of the subject. In both types (clockwise and anti-clockwise) the drawing of the second half is accompanied by less certainty than the first. In Series II, the anti-clockwise circle, a point of maximum uncertainty is sometimes located, and when it is thus mentioned,

as it is in six of the experiments, it is always located as either in the third quarter of the circle or between the third and fourth quarters, at the place where the figure III appears on a clock-face. In Series IIa (clockwise) a point of maximum uncertainty is sometimes mentioned; but it does not show any uniformity in the place at which it occurs, except that it is usually somewhere in the second half of the drawing.

The accuracy and time measurements for Series II and IIa are compared in Table I.

TABLE I

Subject A

	Axes				Time, in seconds				
	II		IIa			II		IIa	
No.	Lgth.	M.V.	Lgth.	M.V.	Quarter		M.V.		M.V.
1.	21.8	2.35	21.1	2.01	1st	2.91	.62	3.39	.58
2.	24.	2.35	21.5	2.72	2d	2.45	.40	2.73	.31
3.	23.8	2.69	21.5	2.87	3d	1.54	.30	2.57	.40
4.	21.6	3.12	21.9	2.25	4th	3.26	.43	2.91	.45
Ave.	22.8		21.5		Total	10.16	1.41	11.60	1.07

E. to E. II. 6. IIa. 3.7

It will be seen from this table that the circles are very nearly the same size. Their average diameters differ by only 1.3 mm. They differ, however, in shape, the anti-clockwise circle being slightly elongated along its first diagonal axis. The clockwise circle shows less difference in the lengths of its axes, and is more nearly round. The distance from end to end is less too. Thus, the anti-clockwise circle is a little more accurate than the other in size, while the clockwise one is slightly more accurate in shape. So the less certain circle is a little less accurate than the other in size, but more accurate in shape.

We turn now to the consideration of the time records. It will be seen from the table that the four quarters of the circle vary a great deal from each other in the speed with which they are drawn. In the anti-clockwise circle the drawing begins slowly in the first quarter, becomes somewhat faster in the second, is fastest in the third, and then in the fourth is slower than ever before. Thus the maximum change of speed comes between the third and fourth quarters, and the change is a retardation. In the other circle we find smaller differences between the times

of the respective quarters. The third quarter is again the fastest, but the difference between it and the fourth is not as great. Instead, the greatest difference is between the first and second quarters, but this too is smaller than that between the third and fourth quarters of the other circle. There the difference was 1.72 sec., here it is only .66 sec. Moreover, the change in the first case is a decrease, here it is an increase in speed. In connection with these characteristics of the time records it will be remembered that in the anti-clockwise circle the point of maximum uncertainty, when it was located, came in the third quarter or between the third and fourth. Also, that while less certain as a whole than the anti-clockwise circle, the clockwise one does not show any definite point of maximum uncertainty. Thus, in so far as we have been able to locate a point of maximum uncertainty, that point comes at the place where the greatest change in speed is taking place, and that change is a decrease. In general the clockwise circle is less certain than the anti-clockwise one, and the table shows that it is drawn more slowly. The second half of each type of circle is less certain than the first, and the table shows that in each case the first half is drawn at an increasing, the second half at a decreasing rate. The point of maximum uncertainty comes in the anti-clockwise circles at the point where the figure III is located on a clock-face. This is the same point with reference to the hand as the end of the first quarter in the clockwise circle. In the first case uncertainty is reported, in the second certainty. In the first case there is a greater change in rate and it is a decrease, whereas in the second case the change is smaller and it is an increase.

The pressure records show certain similar characteristics that hold throughout. In the records for both circles there is a gradual increase of pressure at the beginning, which lasts as a rule throughout the first half, and sometimes as far as the middle of the third quarter. When the third quarter is reached there is usually a decrease in the pressure curve of the anti-clockwise circle. This decrease appears in fourteen of the twenty drawings of Series II. In two drawings it does not appear, and in the remaining four the records are defective. This decrease in pres-

sure comes at the same part of the circle as the great decrease in rate revealed by the time records. It is also at the place at which the maximum uncertainty is reported when it is reported at all. In the two cases where there is no decrease in the third quarter the introspections show that the circles are unusually certain ones. Thus, the decrease in rate and pressure between the third and fourth quarters seems connected with the appearance of uncertainty. In Fig. 5, No. 1, we have a tracing of a curve of this sort from Series II. This curve is reduced in its length but not in its height. The differences in the height of the original curve are so slight compared with its great length that they would not show well if the reduction were made proportional throughout. All the tracings in this article, however, except those in Fig. 5 and two in Fig. 6, are made proportional throughout. Reducing is done by means of a pantagraph. In all the curves a rise indicates an increase in pressure.

The pressure curves of Series IIa, the clockwise circles, show much the same characteristics as the others in their first halves. There are, however, a somewhat larger number of changes in pressure. The second half, in six cases out of the twenty, shows a decrease between the third and fourth quarters. In three of these cases it is accompanied by an attitude of uncertainty. In Fig. 5, No. 2, is given a typical pressure curve for a clockwise drawing. It shows in its first half the same general characteristics as the curve for the other circle, but it has not the decrease between the third and fourth quarters. In Fig. 5, No. 3, we have one of the clock-wise circles which differed from the others of the series markedly. The subject said in his introspections on this circle that he started not with the intention of following the model around as he drew, but rather to draw the circle in one quick sweep. The time records show that this circle was drawn nearly twice as fast as the average, and there is less slowing up in the second half. The result was that the changes in attitude were reduced in degree. The difference in certainty between the first and second halves was less markd. The curve shows scarcely any difference from beginning to end. So we find that in this experiment where the typical difference in certainty is reduced,

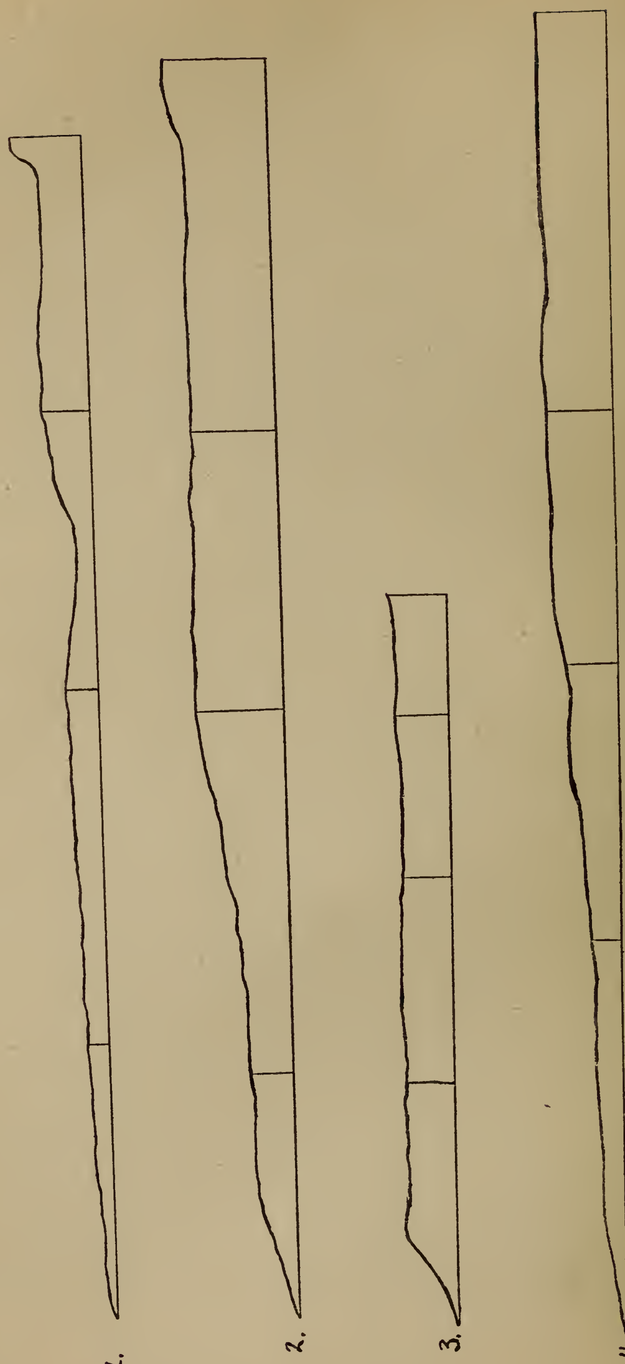


Fig. 5.

the difference in the character of the pressure curve for the two halves is also reduced.

Summing up the results for Series II and IIa, we find that:—

(1) The anti-clockwise circle, which the subject said at the start would be more natural for him, is found in general to be drawn with more certainty than the other one. The first half of each circle is more certain than the second half.

(2) The anti-clockwise circle is more accurate in size, but less accurate in form than the clockwise one.

(3) The clockwise circle is drawn at a slower rate than the anti-clockwise one. In each circle the first half is drawn at an increasing rate and the second half at a decreasing rate.

(4) The pressure curves for the two circles are similar in that there is a gradual rise throughout the first half. The second halves of both curves differ from the first halves. In the anti-clockwise drawings there is a decrease of pressure between the third and fourth quarters. In the clockwise ones this decrease sometimes appears, but there is no regularity about it.

(5) Attitudes of uncertainty go with the second halves of the circles, and especially with the middle of the second half of the anti-clockwise one. At these points there is a decrease in the rate of drawing, and, in the anti-clockwise circle, a decrease in pressure.

At the end of all the experiments a series of ten anti-clockwise circles was made, Series IIb. The results of this series appear in Table II, which follows the plan of Table 1.

TABLE II Subject A

Axes			Time		
No.	Lgth.	M.V.	Quarter		M.V.
1.	14.4	1.2	1st	3.88	.51
2.	15.1	1.1	2nd	2.89	.45
3.	14.5	1.2	3rd	2.12	.26
4.	14.4	1.2	4th	3.30	.38
Ave.	14.6		Total	12.19	1.12

This table shows that the circle is now drawn more accurately as far as its shape is concerned, but it is made much too small. The subject gave no indication in the introspections that he was aware of the small size of his drawing. This variation was intro-

duced primarily to see if there would be any indication of practice effect in the results, and what the effect of this would be upon the attitudes reported. The subject noted some indication of practice effect. This did not appear immediately, but was reported first in the fifth drawing of the series.

In general the drawings of this series are more certain than those of Series II. The subject is able to tell more clearly just what he has done, he reports more attitudes of both positive and negative certainty, and fewer attitudes of uncertainty. There is no longer a specific place at which the maximum uncertainty appears. It is noted only once, and there it is in the fourth quarter. An examination of the time records shows that the drawings as a whole are now made more slowly than in the other series, and there is less difference in time between the third and fourth quarters.

The pressure records of this series are not very satisfactory, owing to the very slight pressure with which the subject drew. Four of the ten are not good enough to give results. In the other six the pressure is fairly constant in its increase from beginning to end, or else it increases quickly at the beginning and maintains about the same level to the end. There is never a decrease between the beginning and the end. One particularly certain drawing was obtained—one in which no uncertainty is reported anywhere in the course of it. The pressure curve for this drawing is illustrated in Fig. 5, No. 4. It will be observed that it rises gradually throughout, with no marked changes, and never a decrease.

In this series, therefore, we find that with the greater certainty revealed in the introspections there is a change in the time and pressure characteristics, this change being in the direction which we have already found to tend toward certainty. The circle as a whole, however, is drawn more slowly than before. Previous results have seemed to indicate that certainty goes with a fast rate of drawing. Thus it would seem that in a long drawing of this character the rate of the total drawing is less important for the attitude than relative rates of the different parts.

Subject C.—With C., the model in Series II and IIa consisted of an equilateral triangle, the sides of which were 28 mm. in

length. This is No. 3 in Fig. 4. In Series II the subject was instructed to draw the triangle beginning at the apex, then drawing obliquely downward to the left, then horizontally to the right, and then obliquely upward to the left to the apex again. Twenty drawings were made in this way. In Series IIa the subject began to draw the triangle at the lower left-hand corner, going first along the horizontal line to the right, then obliquely upward to the apex, and obliquely downward to the starting-point again.

This figure proved to be the least satisfactory of all those used. The drawing of it is not really a continuous process like the drawing of the circle, because the subject pauses at the corners. On the other hand, the three lines are not repetitions of the same mechanical process, as are the three vertical lines of the practice series. That these characteristics are unfavorable to the appearance of contrasts in attitude which can be reported, is shown by the character of the introspections. There was no continuous, changing attitude as in the case of the circle. On the other hand, the lines of the triangle were not compared with each other as were the lines of the practice series. Thus, different parts of the same figure cannot be compared, and the only comparison the introspections allow is of one whole figure with another, or a certain part of a whole figure with a similar part of another.

The method of measurement was to measure the lines and angles of the drawn triangle and the amount by which the end failed to meet the beginning. The lines are numbered 1, 2, and 3 in the order drawn in each case. The angles are similarly numbered.

Table III gives the accuracy and time records for Series II and IIa. The average of twenty drawings is given in each case. The length of the lines is given in millimeters, as is also the distance E. to E. The angles are given in degrees.

Table III Subject C.

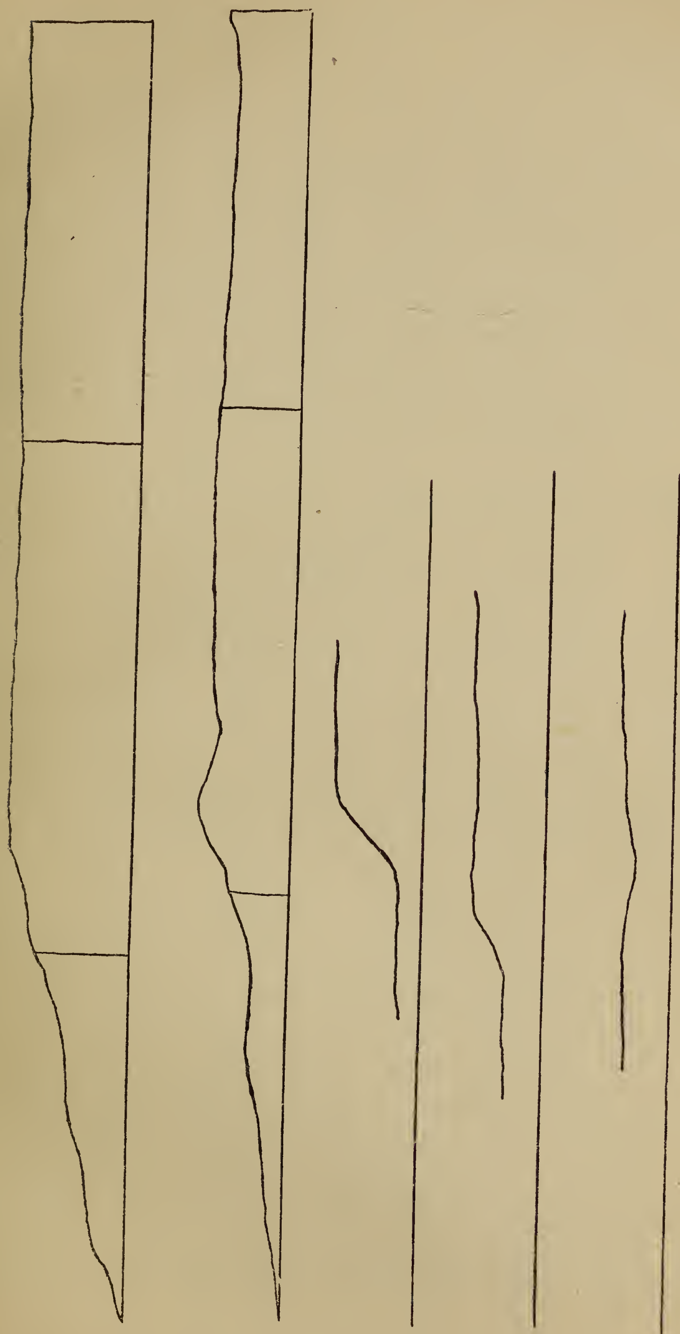
II						
No.	Lgth.	M.V.	Angles.	M.V.	Time	M.V.
1.	25.4	3.7	55.8	1.8	2.30	.57
2.	36.4	5.9	70.9	3.9	2.67	.63
3.	30.4	3.3	57.2	2.6	2.14	.91
	E. to E. 16.5				Total 7.11	1.39

IIa

No.	Lgth.	M.V.	Angles.	M.V.	Time	M.V.
1.	43.8	5.1	79.9	3.8	5.40	1.46
2.	34.7	5.9	47.	4.7	4.73	1.69
3.	40.3	3.3	50.3	4.5	4.90	1.57
	E. to E. 14.5				Total 15.03	4.66

The large mean variations shown in this table indicate that there was very little uniformity in the drawing in either series, especially in the time. This makes it impossible to draw any conclusions, and owing to the difficulty of getting definite introspective comparisons between well-defined parts of these drawings, it is not possible to derive much of value from the pressure records. The only possible correlation between certainty and pressure in Series II is a very broad one. Of the fourteen cases in which drawings had been designated as certain as a whole or uncertain as a whole, five were called certain and nine uncertain. As a whole the pressure curves of the certain drawings are more smooth and even than those of the uncertain drawings. The changes in pressure are more gradual and less frequent in the certain drawings than in the others. The difference between the certain and uncertain curves is not very great to be sure, but it holds in all but two cases out of the fourteen. Fig. 6 gives in No. 1 a tracing of a typical curve for a certain drawing, and in No. 2 one for an uncertain drawing. These curves, as in the case of the circles, are so long that a reduction of their height in proportion to the reduction of their length would make the changes in height very small. They are therefore reduced more in length than in height.

In Series IIa no such comparison of whole drawings is possible as the subject there gives detailed introspections of the process, but does not give an indication of an attitude toward the figure as a whole. There is, however, one very marked peculiarity which appears in some of the pressure curves. This is a sort of "plateau" at the end. It will be remembered that in this figure the end comes at the lower left-hand corner, and the last line is the downward diagonal from upper right to lower left. At the end of this line and completing the figure, this plateau occurs in eight out of the twenty cases, being absent in the other twelve.



1. 2. 3. 4. 5.

FIG. 6.

In Fig. 6, No. 3, a marked plateau is illustrated. No. 4 is an ending of the same sort, but not as marked, and No. 5 illustrates the end of a curve in which there is no plateau. The reduction of these is uniform throughout. The introspections show that in each drawing where a plateau appears at the end of the pressure curve there was an attitude of certainty at the instant of ending. This attitude appears in all but two of the eight cases where the curve shows a plateau, and it appears in only three of the other twelve drawings of the series. This attitude often gives way immediately to a reflective attitude of uncertainty toward the process as a whole, and this attitude of uncertainty seems closely bound up with the visual image which comes up just after the completion of the drawing.

Subject F.—The model used with F. in Series II consisted of four slant lines, 27 mm. in length, put together somewhat in the form of an M. For Series IIa the model was the same figure inverted. The first figure is illustrated in Fig. 4, No. 4, and the second one in Fig. 4, No. 5. In both these figures the subject was instructed to draw the lines all from top to bottom, this being the way that seemed most natural for him. There are certain similarities and certain differences in the process of drawing these two types of figures. Lines 1 and 3 of the first type correspond to lines 2 and 4 of the second in that they are drawn from upper right to lower left, and are parallel to each other. Similarly, lines 2 and 4 of the first figure correspond to lines 1 and 3 of the second in that they are drawn in the other oblique direction and form a parallel pair. The lines of both figures are the same in that they are drawn from top to bottom in each case and are slanted at the same angle from the perpendicular. An important difference between the two figures is that in the first the subject is drawing down *from* points, while in the other he is drawing down *to* points. There is a difference in the mechanical difficulty of drawing a line from upper right to lower left and in the other oblique direction. The former direction the subject pronounced the easier, mentioning it several times in the course of the series.

The method of measurement was as follows. First, the length

of each line was measured in millimeters. Then the straightness of the line was indicated by a figure expressing the number of slight deviations that occurred in it. The direction of the line was determined and expressed in terms of the number of millimeters by which the line deviated from the correct slant. Finally, the amount by which the successive lines failed to meet each other at the points was also determined in millimeters.

For time measurement the four lines were divided into halves. The time was determined in each line, first for the period at the start during which the pencil was down but not moving, then for each of the two halves, and then for the period at the end during which the pencil was at rest. The time elapsing between the end of one line and the beginning of the next, or the time in the points, was also determined.

Table IV gives the accuracy averages for the twenty drawings

TABLE IV

Subject F.

II				
No.	Lgth.	Str.	Dir.	Points
1.	30.9	1.1	1.6	5.72
2.	23.9	1.7	1.3	4.75
3.	26.5	1.2	3.2	3.25
4.	21.7	2.1	1.3	

IIa				
No.	Lgth.	Str.	Dir.	Points.
1.	29.0	2.0	4.3	4.05
2.	26.3	1.6	2.3	3.22
3.	22.6	1.5	2.5	4.45
4.	28.1	1.9	2.3	

of each series. It will be seen from this table that in Series II, lines 1 and 3 are more accurate, except in direction, than 2 and 4, while in Series IIa just the opposite is the case. So, the lines which are mechanically easiest to draw are those which are drawn most accurately. There is a certain correspondence between parallel pairs in the same figure, and in each series line 1 is drawn longer than any of the others.

The time records for Series II and IIa are given in Table V—in each case the average of twenty experiments. The smaller time determinations—those for the parts of the lines—do not

show any marked uniformities and are omitted from this table. Under "Time" is given the average time for each line. Under "Rate" is given the average rate at which it is drawn. The rate is determined by dividing the length of each line by the time it took to draw it. This gives the rate in millimeters per second, and the average of these determinations appears in the table. Under "T. P." is given the average time between the ending of one line and the beginning of the next.

Of the columns in this table those for rate are the most important. In II the rate for line 1 is higher—a greater number of millimeters per second—than that of any of the other lines. As a pair too, 1 and 3 are more rapidly drawn than 2 and 4, just

Table V

Subject F.

II

No.	Time	M.V.	Rate	M.V.	T.P.
1.	4.27	.38	7.40	.96	.92
2.	4.32	.37	5.67	.87	1.08
3.	4.10	.46	6.47	1.03	.82
4.	4.44	.43	4.85	.57	

IIa

No.	Time	M.V.	Rate	M.V.	T.P.
1.	4.36	.33	6.69	.65	.84
2.	3.69	.36	7.05	.77	.86
3.	3.86	.33	5.92	.56	.89
4.	3.68	.31	7.67	.92	

as in the accuracy table they are found to be drawn longer. In Series IIa we find that 2 and 4 are more rapidly drawn than 1 and 3, though 1 is drawn fairly fast. The accuracy table shows that these fast-drawn lines are long, so that in general the tendency to draw at a fast rate and the tendency to make the line long go together.

From the introspections it is possible to get in a general way the order of certainty of the four lines in Series II and Series IIa. There is sometimes enough difference between the four lines introspectively to enable the subject to name them in their order of certainty. Where this is not given it is often possible to tell from the general character of the introspective report what this order

was. The order of certainty most frequently given in Series II was 1, 3, 2, 4. This order was given five times. The order of certainty is given in all only nine times, and no other order of certainty appears more than once. This order receives support also from the introspections in which the certainty of one or two lines is spoken of, but the complete order not given. Line 1 is called most certain in every case, line 4 is called less certain than line 2 in all but two cases. The general outcome of the introspections of Series II, as far as the order of certainty is concerned, is that line 1 is the most certain line, that 1 and 3 are more certain than 2 and 4, and that the most usual order of certainty is 1, 3, 2, 4.

Recurring for a moment to the accuracy and time records, we find that the most certain line 1 is the longest and most rapidly drawn. Line 3 is next in length and rapidity, and then follow lines 2 and 4 respectively.

The introspections of Series IIa are somewhat more definite than those of Series II. Line 1 is again most certain in all but two of the twelve cases in which the order of certainty is given. Lines 2 and 4 are much more certain in this series than in the other one, and line 3 is called least certain every time it is compared with the others. The typical order of certainty for this series is 1, 4, 2, 3.

Turning to the records of accuracy and time for this series, we find that the certainty of line 1 and of lines 2 and 4 goes with greater length of line and faster rate of drawing. The greater length and faster rate of line 1 is, however, not as marked here as in the other figure where it is drawn in the other easier oblique direction. Lines 2 and 4, which are more certain in this figure than in the other one, are also drawn at a faster rate here than they were there. Line 3, which is least certain in this series, is drawn most slowly and is shortest. So it tends to hold here too that certainty goes with a long line rapidly drawn.

One other point must be considered before we go on to the pressure records. It was emphasized above that lines drawn from upper right to lower left were mechanically easier for the subject to execute than the lines drawn in the other oblique

direction. Does certainty simply go with this mechanical ease of drawing? In Series II it evidently does. Lines 1 and 3 are more certain than the mechanically harder lines 2 and 4. If it is true of the other series as well, lines 2 and 4 should be more certain than lines 1 and 3. We find that they are more certain than 3, but not as certain as 1. If we should try to show a correlation between certainty and the mechanical ease of drawing, we would be unable to explain how line 1, mechanically easy in one case and mechanically difficult in another, is yet most certain in each case. The fact that it is the first line drawn in each case doubtless has something to do with it, but this will not help our explanation unless the fact that it is the first line drawn makes the process of drawing it characteristic in some way that will show in our records. In the cases of the other lines we have found a correlation of certainty with length of line and fast rate. This does not hold in the case of line 1 in Series IIa in comparison with lines 2 and 4 of its own series, for although it is longer and drawn at a faster rate than the other three lines which are drawn in the same direction (II, 2 and 4, IIa, 3) it is not drawn faster than, nor is it much longer than, lines 2 and 4 of its own series. Its certainty must therefore be connected with some other factor which we have not yet observed.

We find such a factor the moment we examine the pressure curves. The pressure curves for line 1 conform to the same type throughout both series, except two cases in Series IIa where alone line 1 is called uncertain. The form of the curve is a gradual steady rise to a maximum which is held without decrease to the end. Fig. 7 shows two curves (Nos. 1 and 2) for line 1, the first from Series II, the second from Series IIa. No. 3 is a curve from one of the two drawings in Series IIa in which line 1 was called uncertain. Its variation in form is marked. We find here, therefore, in line 1, that certainty and a definite type of curve go together, and that this holds regardless of the mechanical ease or difficulty with which the line is drawn.

In the later lines of the drawings the curves show greater pressure than in line 1, and a somewhat different shape. They rise more quickly and their increase is not quite as uniform. Fig. 7,

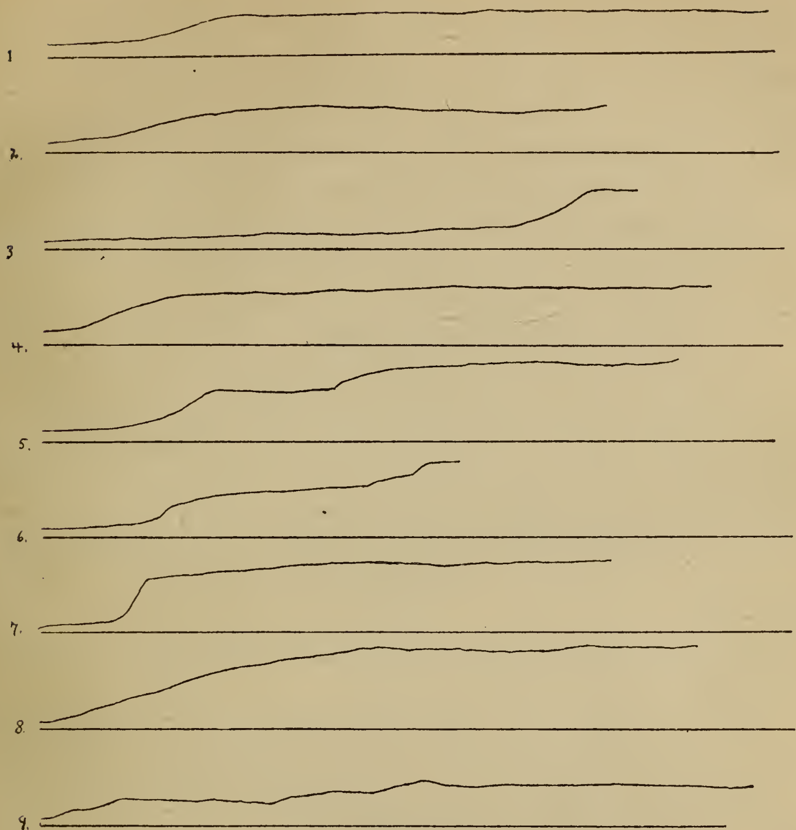


FIG. 7.

No. 4, shows a pressure curve from line 2 in the first figure (Series II) in which 2 was called next to 1 the most certain line in the drawing. Below it is given in No. 5 an example in which this line was called least certain of the four. Fig. 7, Nos. 6 and 7, show examples of line 3 in Series IIa. In 6 the line was certain, in 7 uncertain. In 8 and 9 of Fig. 7 there are illustrated two pressure curves of line 4 in the first figure (Series II). In No. 8 line 4 was certain, in No. 9 it was uncertain.

This correlation between certainty and a definite type of curve, and uncertainty and a variation from that type holds in every case for line 1 in both figures. It also holds where a line, usually certain, is pronounced particularly uncertain. But it does not

hold between small differences in certainty. There are examples in the records of both figures where there is no appreciable difference between the curves of lines 2 and 3, and yet one is called more certain than the other. Evidently, then, only wide contrasts in subjective certainty show corresponding differences in the pressure records under the conditions of this experiment.

Summarizing the results of this experiment with subject F. we find that:—

(1) In general certainty goes with a longer line and a faster rate of drawing than does uncertainty.

(2) The lines which are mechanically easy for the subject to execute are usually the lines drawn in this way.

(3) Line 1 is most certain in each figure, though in the first it is mechanically easy and in the second mechanically difficult. This certainty is correlated with a definite form of pressure curve which is followed in the drawing of line 1 in both figures, and is departed from only in the two cases in which line 1 is called uncertain.

(4) In the other lines marked contrasts in subjective certainty are reflected in the pressure curves.

Subject R.—In Series II the experiment with R. was identical with that of the practice series after the preliminary line had been added to the other three. It consisted in drawing the four parallel vertical lines from top to bottom. In Series IIa a variation was introduced. This was simply that instead of being drawn from top to bottom the lines were drawn from bottom to top.

One change was made in the method of measurement. Instead of dividing the lines into thirds and finding the time for each third, the lines were divided into halves. Twenty drawings were made in each series, and the usual full introspections were taken. The subject was able in almost every case to name the lines in their order of certainty. No uniform order of certainty developed. The succession of drawing, or the position of a line in a drawing had no effect upon its certainty or uncertainty.

A comparison of the accuracy and time averages of Series II and Series IIa shows that the lines when drawn from bottom to

top (IIa) are longer, less straight (though as good in general direction), and more slowly drawn, than the lines of the other series where they are drawn from top to bottom. These differences cannot be correlated with a general difference in certainty between the two sets of lines. The introspections reveal no general difference which would enable us to say that one series had been more or less certain than the other. Yet, since the two series show such marked differences in the accuracy and time records, we must keep the two series separate when we compare the records of the most certain lines with those of the least certain ones, as we now proceed to do.

The averages of the most certain lines and least certain lines are given in Table VI. The smaller time determinations are omitted as they show no differences between the most certain and least certain lines not expressed in the time for the whole line. The average rate, however, is included in the table.

Table VI
Subject R.

II	Lgth.	M.V.	Straightness	Time	M.V.	Rate	M.V.
Most	25.4	3.1	(.9) 1.	1.07	.22	24.2	4.5
Least	24.6	2.8	(.94) 1.1	1.10	.22	23.0	3.2
IIa							
Most	31.9	2.2	(1.8) 1.3	1.46	.21	22.2	2.2
Least	32.6	1.8	(1.8) 1.	1.57	.24	21.7	3.

In accuracy the most and least certain lines are just about the same. In the time values, however, there is a difference, the most certain lines being drawn at a slightly faster rate than the least certain whether the lines are made from top to bottom or from bottom to top.

As has been said, the subject gave the order of certainty of the lines in every case. This order of certainty was, therefore, compared in every case with the characteristics of the pressure curves. It soon appeared that the line which came first in the order of certainty always had a definite form of pressure curve, and that the other lines differed from this type in a greater or less degree according to their degree of certainty. So marked was this form of curve and so universally did it hold, that it was possible to tell solely from an examination of the pressure curves

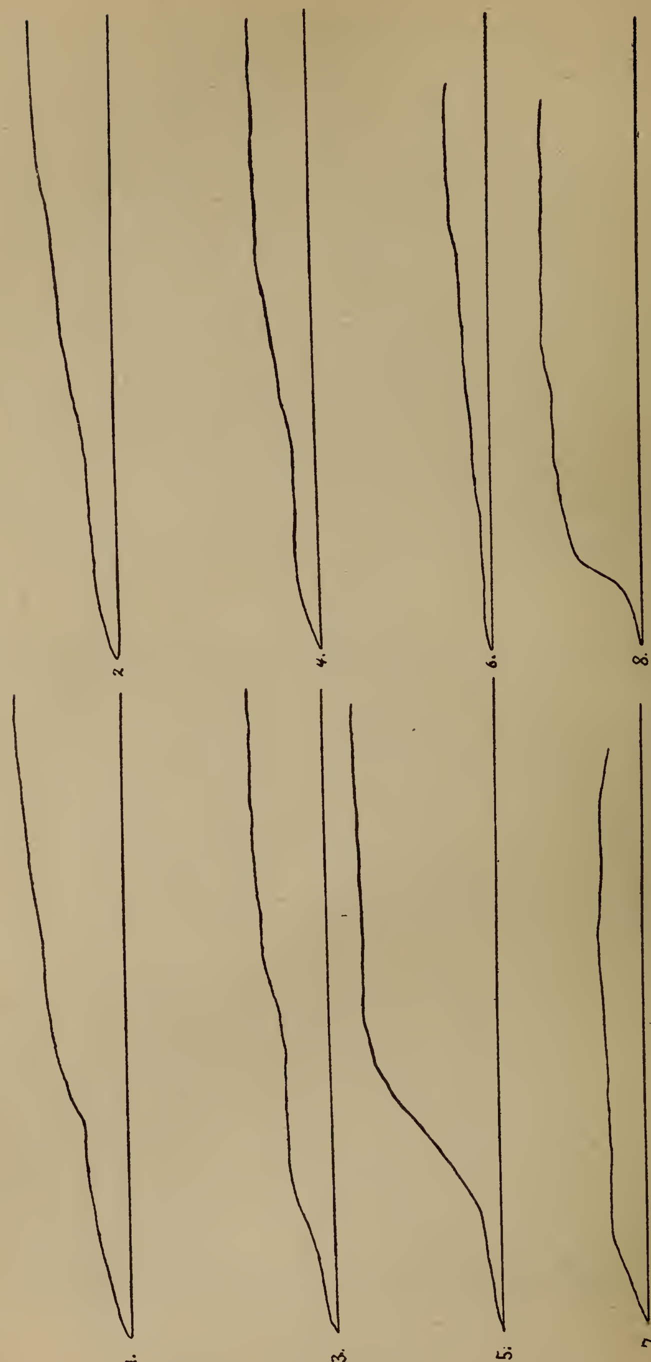


Fig. 8.

which line had been called most certain, and in nearly every case the order in which the other two followed it. This was true both of Series II and Series IIa. The "certainty" form of curve is the same for the drawings of each series. Fig. 8 shows some examples of curves taken from both series. Nos. 1 and 2 are certainty curves from Series II, Nos. 3 and 4 certainty curves from Series IIa. It will be observed that the curves follow the same form whether the lines are drawn from top to bottom or from bottom to top, reversing the mechanical process of drawing. The remaining curves in Fig. 8 are tracings of uncertainty curves from both series. They vary from the certainty type in two ways. Either the increase at the start is too rapid, as in No. 5, or the increase is more gradual as in No. 6. No. 7 is a curve from a line which the subject called "uncertain at the end." This curve shows a decrease of pressure at the end.

Not only does the curve for certainty follow a definite form, but it shows a regularity in its height as well. In the great majority (80%) of the cases the pressure curve for the most certain line is between the curves for the other two lines in height. So there is a general tendency for the certain line to be that one which is drawn with a medium pressure.

This experiment, therefore, has shown certainty in a certain part of the drawing process going with a fast rate of drawing in that part and with a definite form of reaction as shown by the pressure curve. The accuracy results which show no difference in accuracy between the certain and uncertain drawings are interesting as showing that an accurate drawing does not necessarily represent the kind of reaction characterized by certainty. The accurate drawings vary as to certainty or uncertainty with the character of their pressure curves. The same is true of the inaccurate ones.

Series III

In this series no model was used and the subjects were all given the same task. They were instructed to make a dot, raise the pencil and move it to a point perpendicularly above the dot, and draw a straight line down to, and ending in, the dot. The distance above the dot to which the pencil should be moved was

not designated. The records of the process included everything from the making of the dot to the end of the downward stroke. After a series of ten experiments had been made in this way, the subject was given special instructions each time as to the rate at which he should draw the downward line. Sometimes he was told to draw it slowly, at other times rapidly, and at other times at his natural speed. The instruction changed from experiment to experiment. This enabled the subject to get an introspective comparison between the experiences of drawing at different rates. About ten experiments were made with each of these special instructions, so that in this series there were in all about forty experiments with each subject.

The measurements for accuracy included the length of the line in millimeters, its straightness, and the amount by which it varied from the vertical where there was such variation. Where the line failed to strike the dot the horizontal and vertical distances from the end of the line to the dot were recorded in millimeters. The time measurements included the time consumed in making the dot, the time between the making of the dot and the beginning of the downward line, and the time for the downward line with the usual minor determinations.

The purpose of this arrangement is to provide a drawing process which shall be as simple as possible. The downward line is now drawn with but one determining purpose—to reach the dot. The experiments with the different subjects will be considered separately.

Subject A.—A. reports a good deal of visual imagery. Attitudes of certainty and clear visual imagery go together. This fact is observed and commented upon by the subject. He said once when he gave his introspections for an uncertain drawing, "I always feel when a line is uncertain that the visual scheme is weak."

The tables in this series give the number of attitudes of certainty and uncertainty reported by the subject in each one of the four parts of the series. They also give the average length, time, and rate of the lines for the cases of certainty and uncertainty respectively.

TABLE VII
Subject A

No special instruction

Attitude	No.	Length	Time	Rate
Certainty	4.	21.1	3.33	6.47
Uncertainty	6.	23.	3.19	7.35

Special instruction:—Natural speed

Attitude	No.	Length	Time	Rate
Certainty	3.	18.5	2.49	7.37
Uncertainty	6.	20.7	2.60	8.71

Special instruction:—Slow

Attitude	No.	Length	Time	Rate
Certainty	4.	18.9	4.20	4.46
Uncertainty	5.	23.4	5.13	4.70

Special instruction:—Fast

Attitude	No.	Length	Time	Rate
Certainty	2.	16.5	1.21	13.6
Uncertainty	1.	17.	1.30	13.
No attitude	3.	16.6	.99	18.4

This table shows no marked correspondence between attitude and length, or between attitude and time. If one rate—either fast or slow—were especially favorable for the appearance of certainty we should expect a larger percentage of attitudes of certainty in the experiments in which the instruction to draw at that rate was given. In general the certain drawings are drawn at a somewhat slower rate than the uncertain ones. The special instructions do not greatly affect the relative number of attitudes of certainty and uncertainty which appear. In the fast series when the rate becomes very fast no attitude appears.

As for the accuracy with which the subject carried out his task, the measure for this is given in the average horizontal and vertical errors. The horizontal error is the distance from the dot to the downward line, and the vertical error the vertical distance between the end of the line and the level of the dot. The average errors for all cases of certainty and all cases of uncertainty are as follows:

	Horizontal	Vertical
Certainty	.8	1.9
Uncertainty	.5	3.3

This shows that in cases of certainty, on the average, the end of the line comes nearer to the level of the dot. There is only

a slight difference in the horizontal error, it being slightly greater for the certain lines.

The pressure records in this series for this subject are very unsatisfactory, because he drew with such very light pressure that in many cases the rise in the curve was barely perceptible. No correlation is possible, therefore, between certainty and a definite type or amount of pressure. One experiment in this series, however, is especially interesting. In one drawing the subject was instructed to draw at the rate which would give him most certainty and "exert a little more pressure." The result was that the subject, following these instructions, drew with great certainty. He pronounced it "as good certainty as I've had."

In general, however, we cannot draw any conclusions from this series with this subject except the one derived from the introspections, that an attitude of certainty is always accompanied by a clear visual image.

Subject C.—With C. the introspections are quite different in character. He relies mainly upon kinaesthetic factors to guide him in the drawing process. Table VIII follows the plan of the preceding table.

TABLE VIII
Subject C

	No special instructions			
Attitude	No.	Length	Time	Rate
Certainty	10	18.9	2.60	7.0
Uncertainty	0	—	—	—
	Special instruction:—Natural speed			
Attitude	No.	Length	Time	Rate
Certainty	6	16.8	2.03	8.3
Uncertainty	0	—	—	—
No attitude	1	18.	1.52	11.8
	Special instruction:—Slow			
Attitude	No.	Length	Time	Rate
Certainty	2	13.5	4.04	3.33
Uncertainty	8	15.8	6.46	2.52
	Special instruction:—Fast			
Attitude	No.	Length	Time	Rate
Certainty	9	19.	1.13	18.6
Uncertainty	1	17.	.65	26.1

It is apparent at once from these tables that with this subject the rate of drawing has a decided influence upon the attitudes of certainty. His normal rate, as shown by the drawings made with no special instructions, is accompanied by certainty in every case. In the experiments with special instructions the ones in which he is instructed to draw at natural rate are faster than those in which no special instructions are given. In the experiments with the instruction to draw slowly the drawing is made very slowly, except in two cases in which the subject reported that he had forgotten the instruction. Great uncertainty was reported throughout these drawings, except in the two cases in which the instructions had been forgotten. The instruction to draw fast increases certainty. The fast drawings are all called particularly certain by the subject—more so even than those made with no special instructions. Only one drawing in this series shows a trace of uncertainty. This is the fastest of all the fast drawings, and we shall have occasion to refer to it again when we consider the pressure records.

The horizontal and vertical errors for the different series are as follows:

	Horizontal	Vertical
No special instruction	1.35	1.3
Instruction—Natural Speed	.60	.8
Instruction—Slow	1.20	3.5
Instruction—Fast	1.25	.85

The smaller error in the Natural speed series may be attributed to practice, since these were made after the drawings with no special instructions. The slow series in which all but one of the cases of uncertainty occur, shows a large vertical error, and so a correlation between uncertainty and inaccuracy. The inaccuracy is due to the fact that the subject stops drawing before he has come down to the level of the dot. In his introspections he says in one place that "in the slow drawings the length of the line seems greater." Evidently, then, when he draws slowly he overestimates the length of the line he is drawing.

The lines drawn with no special instructions were all certain, and their pressure curves all conform to a fixed type. This curve rises slowly and then shoots up to form a plateau at the end.

This type of curve was found to go with an attitude of certainty when the triangle was used as a model and the last stroke was a downward one. A typical example is given in Fig. 9, No. 1.

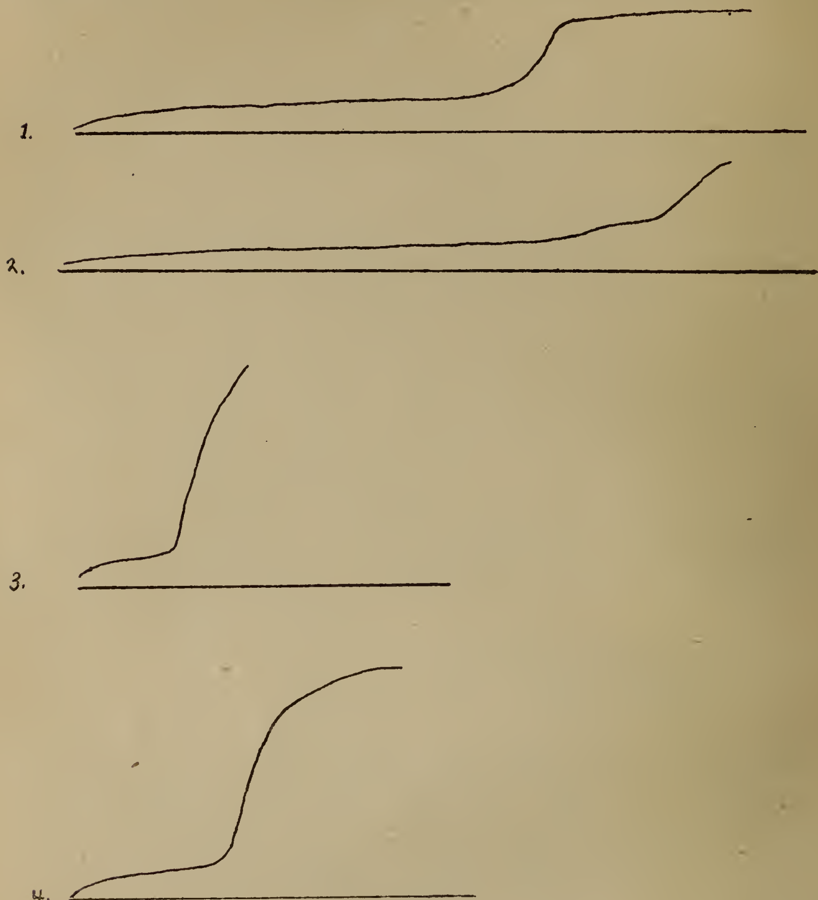


Fig. 9.

In the series with the special instructions to draw at natural speed, the curve continues to show the certainty type in three of the seven cases, but in four cases it is different. The curves of these four cases are like each other, however, and one of them is illustrated in Fig. 9, No. 2. The introspections do not show that the drawings for which these are the records are markedly different from the others except that they are somewhat less

certain. In one case certainty was reported. In another there was no attitude. In a third, certainty was reported, but it was called less in degree than that of the previous experiment, the curve of which had a plateau. In the fourth case the subject said that the line had run out of true about half-way down, but had come back, negative certainty giving way to positive.

The curves for the slow drawings may be briefly dealt with. The pressure is very slight in every case, and the plateau at the end is present in only one case. Even here it is so slight as to be scarcely noticeable. So we find that the following out of the special instruction to draw slowly has affected not only the attitude of certainty with which the drawing is made, but also the amount of pressure exerted and the form of the curve.

In the fast drawings the typical certainty curve is present in all but two cases. The curve in these cases is more like the varying form that was observed in the series with the special instruction to draw at natural speed. One of these is the drawing referred to above as the fastest of all the fast drawings—the only case of uncertainty outside the slow series. This pressure curve is shown in Fig. 9, No. 3. No. 4 shows a typical curve from the fast series.

So we find with this subject that the attitude he reports varies with the rate of drawing, and the type of pressure curve varies with the attitude.

Subject F.—In this series F. reports both types of imagery, visual-motor imagery being most prominent. With him the experiment is the same as with the other subjects and the table follows the same plan.

TABLE IX

Subject F.

No special instructions

Attitude	No.	Length	Time	Rate
Certainty	6	15.4	3.31	4.98
Uncertainty	4	14.5	3.14	4.80

Special instruction:—Natural speed

Attitude	No.	Length	Time	Rate
Certainty	4	20.2	2.83	7.26
Uncertainty	2	18.7	3.66	5.11

Special instruction:—Slow

Attitude	No.	Length	Time	Rate
Certainty	5	20	5.51	3.75
Uncertainty	3	21	5.31	4.04
No attitude	2	20	5.25	3.85

Special instruction:—Fast

Attitude	No.	Length	Time	Rate
Certainty	8	20	1.29	16.3
Uncertainty	2	19	1.64	14.5

These tables show that the subject was not as much affected by the special instructions as was C. The instruction to draw slowly does not result in any marked change in the degree of certainty or in the number of times it is reported. The instruction to draw fast, however, has such an effect, which is to make the series in general more certain. The two uncertain drawings were the first of the series. The subject had a little trouble in getting accustomed to drawing the line fast, and this is reflected in the uncertainty of the first two drawings. After these, however, the series is composed entirely of certain drawings, and the degree of certainty is great.

With this subject the greater average errors are in the certain drawings, so certainty and accuracy in this case do not go together. The average errors are as follows:

	Horizontal	Vertical
Certainty	.8	2.9
Uncertainty	.65	2.3

The pressure records with this subject show little difference between the certain and uncertain drawings when there are no special instructions, or when the instruction is to draw at natural speed. On the whole the curves of the certain drawings are more regular than those of the uncertain ones, but the difference is not great enough to show a definite type of curve for certainty, from which the uncertain curves vary. Six of the ten certain drawings show curves with characteristics similar enough to warrant our calling them all of the same form, but three of the six curves from uncertain drawings follow this form as well. Fig. 10, No. 1, illustrates a curve of this type from a certain drawing. When we come to the special instructions "slow" and "fast," however, we find that the certain and uncertain drawings have

different pressure curves. The curves for the certain drawings conform to the type found in the previous drawings, while the uncertain ones vary from it in some way. In the slow series the curves for the two drawings in which definite uncertainty was reported are very flat, showing very light pressure and very little difference in pressure between the beginning and the end of the line. They are less in amount than any of the certain drawings except one in which "only fair certainty" is reported. Fig. 10

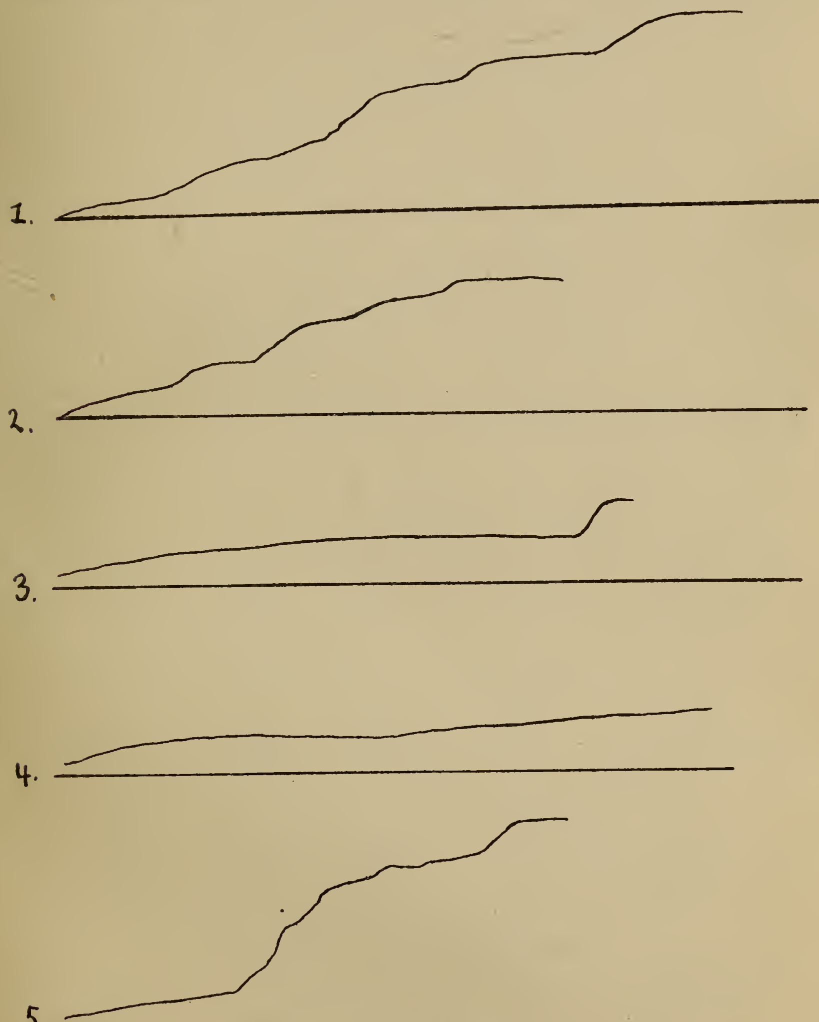


Fig. 10.

gives in Nos. 2, 3, and 4 respectively, tracings from a certain, an only moderately certain, and an uncertain drawing.

In the drawings with the instruction "fast," certainty was reported in all but the first two cases, in which the subject was becoming accustomed to the process. All the curves of this series except the first two conform to the type already found to accompany certainty. One of them is illustrated in Fig. 10, No. 5.

With this subject, therefore, we have not been able to correlate the attitudes of certainty and uncertainty with a definite form of curve when the subject has chosen his own rate of drawing, or has been instructed to draw at natural speed. When, however, he has been instructed to draw slowly or at a fast rate, differences in the objective records appear, and these vary with differences in attitude.

Subject R.—R. differs from the other subjects in the amount and kind of imagery he reports. His imagery is chiefly visual, though kinaesthetic imagery sometimes appears. Often incomplete images are reported, and sometimes part of an image is vague while the rest is clear.

Table X

Subject R.

No special instructions

Attitude	No.	Length	Time	Rate
Certainty	7	16.4	1.98	8.9
Uncertainty	3	18.8	2.55	7.9

Special instruction:—Natural speed

Attitude	No.	Length	Time	Rate
Certainty	4	17.6	2.20	8.5
Uncertainty	6	15.8	2.39	6.8

Special instruction:—Slow

Attitude	No.	Length	Time	Rate
Certainty	3	15.6	2.72	5.86
Uncertainty	6	15.3	3.97	4.15

Special instruction:—Fast

Attitude	No.	Length	Time	Rate
Certainty	7	16.	1.00	15.5
Uncertainty	3	13.3	1.20	11.9

These tables show a correlation between the rate of drawing and the character of the attitudes reported. The averages in all four parts of the table show that the certain lines were drawn at a faster rate, and (except with "no special instructions") they are made longer than the uncertain.

The accuracy with which the subject carries out his task seems not to have any effect upon his certainty. A comparison between the average horizontal and vertical errors of the certain and uncertain cases is given in the following table.

	Horizontal	Vertical
Certainty	1.1	2.3
Uncertainty	1.8	1.5

Turning now to the pressure curves of this series, we find that in the experiments with no special instruction, and in the ones with the instruction to draw at natural speed, the certain drawings follow a form which is rather like the form of the curve for the certain lines in the experiment with this subject in Series II and IIa. The curves for the uncertain drawings show a smaller amount of pressure, and they vary from the certainty form in other ways. They are little higher at the end than at the beginning, and sometimes show a decrease of pressure within the drawing of the line. Of the eleven cases of certainty all but four have curves of the certainty form. Among the nine cases of uncertainty only one shows a curve of the certainty form. Fig. 11 shows in No. 1 a curve from a certain drawing and in No. 2 one from an uncertain drawing.

In the slow drawings there are three cases of certainty and six of uncertainty. Two of the cases of certainty have curves of the certainty form, the curve for the third is flat. The cases of uncertainty all have flat curves that show no trace of the certainty form. One of these drawings is peculiar, however, in that it is of the usual flat uncertainty type until the very end. Then there is a little sudden increase in pressure. The curve is illustrated in Fig. 11, No. 3. The introspections on this line show that during the drawing it was attended by an attitude of uncertainty, but that at the very end there appeared an attitude of certainty.

1. 2. 3. 4.

FIG. 11.

The curves for the fast series are all of the certainty type except four. Two of these are curves for drawings which were called certain in spite of this variation. An example of a certainty curve from a fast drawing is shown in Fig. 11, No. 4.

With this subject, therefore, certainty tends again to go with a long line drawn at a fast rate, and a definite type of pressure curve. Uncertainty is correlated, though not perfectly, with a shorter line drawn at a slower rate, and a variation from the type of pressure curve found in the certain drawings. The accuracy with which the subject carries out his task, as measured by the horizontal and vertical errors, seems to have no effect upon the attitude of certainty.

Summarizing the results for Series III for all subjects, we find that there are large individual differences. These appear both in the introspective reports and in the objective records. With A. clear visual imagery and certainty go together. He is much less influenced as to his attitude by varying rates of drawing than any of the other subjects. Since his pressure records were unsatisfactory it is impossible to compare them with those of the other subjects, but it may be remarked that here as elsewhere he draws with lighter pressure than any of them. C. relies almost entirely upon kinaesthetic factors, and with him rate of drawing has a great influence upon attitude, a slow rate going with uncertainty and a fast rate with certainty. Moreover, with this subject certainty goes with a definite type of pressure curve, and when the attitude is modified with change of rate, this form of curve is modified as well. F.'s imagery is more visual than that of any of the other subjects except A. The correlation between attitude and rate is slightly better with him than with A., but not as good as with the other two subjects. With F. it is not possible to correlate the attitude of certainty with a definite form of curve when he has no special instructions or is instructed to draw at natural speed. When, however, he is drawing at a rate faster or slower than the normal, the attitude of certainty is found to be correlated with a definite type of curve. R. reports less imagery than any of the other subjects. He relies more upon kinaesthetic sensations in drawing than any of the others

except C. With him there is found a good correlation between length, time, rate, and attitude. The special instructions have more effect upon the relative number of attitudes of certainty and uncertainty reported than in the cases of A. and F., but less effect than in the case of C. With this subject (R.) there is also a fairly good correlation between certainty and a definite type of pressure curve and between uncertainty and some variation from this type.

It is worthy of special comment that the subjects form a series with regard to the completeness of the correlation between attitude and objective records. They also form a series in the imagery they report, and they are ranked in the same order in each series. With the most visual subject the correlation is least complete, and with the least visual and most kinaesthetic subject the correlation is most complete. It seems, then, that with a visualist the attitude of certainty appearing in the course of a drawing is less influenced by the rate of drawing and the pressure exerted than is the attitude of a subject who relies chiefly upon kinaesthetic factors. With the visualist the important thing for an attitude of certainty seems to be a clear visual image, with little or no relevant change in rate and pressure appearing. With the kinaesthetic subject visual imagery has little to do with the attitude which, however, does vary with the rate and pressure characteristics.

Series IV and IVa

This series, in which all the subjects took part, had for its task the drawing of a simple figure with the left hand. First the drawing was made by the left hand alone (Series IV) and then by the left hand with the right hand drawing symmetrically with it (Series IVa). In this second series the subject took two pencils and made two symmetrical drawings, one with each hand. The figures used are illustrated in Fig. 4, Nos. 6 and 7. The dotted lines in these figures indicate the path followed by the right hand when it drew with the left, though these lines did not appear in the model. The same model, illustrating the drawing to be made by the left hand, was used both when the left hand drew alone and when the right hand drew with it. It was thought

that these conditions would furnish a good contrast between the attitudes which appeared when the left hand drew alone and when the right hand drew with it. As all the subjects were right-handed, drawing even of the simplest figures with the left hand would be a very unaccustomed activity. On the basis of previous experiments it was thought that the process of carrying out this activity, with left hand alone, would be attended by attitudes of uncertainty. It was thought too that when the right hand drew a symmetrical figure at the same time, the better motor control thus gained for the left would result in greater certainty. The good motor control over symmetrical movements is well known.

Accordingly, a series of the drawings from each of two models was first made with the left hand alone. This series was completed before the series of symmetrical drawings was begun to avoid the practice effect which the symmetrical drawings would have had upon the others if they had been intermingled with them. Then the symmetrical drawings from each of the two models were made. The figures were always made continuously, that is without raising the pencil from the paper. They were begun at the top, and the left hand went first to the left and then downward, or, in the case of the second figure, obliquely to the right. In the symmetrically drawn figures the right hand followed a corresponding inverse course.

In this series the usual accuracy, time, and pressure records were taken. In the symmetrical drawings the time and pressure records were taken of the left-hand drawing only, but the right-hand drawing was measured for its accuracy. The symmetrical drawings necessitated a special arrangement of the apparatus. The right-hand drawing had to be made upon a smooth surface exactly beside the left-hand one which was made over the pressure plate. To provide an arrangement which would satisfy these conditions half the opening in the plate, H, Fig. 1, was filled up. A piece of zinc the exact size and shape of the right-hand half of the opening was cut out and soldered in place, so that the zinc simply formed a continuation of the original plate. For the right-hand drawings rectangular pieces of paper were cut out a convenient size and fixed upon this new portion of the plate.

In reporting the results of these experiments the model consisting of a horizontal and a vertical line, Fig. 4, No. 6, will be spoken of as the first model, and the one composed of the two slant lines, No. 7, will be called the second model. The lines of the first model are shorter than those of the second. In the first they are 15 mm. long, and in the second 20 mm. The angle is a right angle in each case.

The measurements of accuracy with the first figure included the lengths of the two lines in millimeters, the usual two determinations of their straightness, and the size of the angle in degrees. In the time records the time for each line was measured and the time for the drawing of the whole figure determined. In the second figure the method of measurement was a little different. The length of the two lines was measured in millimeters. There was only one determination of straightness, however. This was the number of slight fluctuations—the general deviation was not measured. The amount by which the two lines vary in direction with regard to each other is contained, however, in the measurement of the angle. The time measurements include the time for each line and the time the pencil is at rest at the apex of the angle between the two lines.

Table XI compares the accuracy records of the two series with the first model. Opposite "IV" are given the averages for the left-hand drawings when they were made with the left hand alone. Opposite "IVa" are given the averages for the left-hand drawings, and opposite "IVa R. H." the averages for the right-hand drawings, in the symmetrical series. The lengths of the horizontal and vertical lines are given under "L'gth H" and "L'gth V." Their straightness error in terms of the number of little deviations under "Str. H" and "Str. V," and their direction or amount of total deviation under "D H" and "D V." Under "angle" is given the average size of the angle in degrees. The figures in parentheses are mean variations.

TABLE XI

Accuracy (First model)

Subject A

	L'gth.H.	L'gth.V.	Str.H.	Str.V.	D.H.	D.V.	Angle
IV	12.2 (2.)	11. (1.5)	2.5	2.7	1.	2.2	105 (3.)
IVa	11.4 (1.)	11.2 (.5)	2.3	3.	1.8	3.	112 (6.)
IVaRH	10. (1.)	12. (1.)	1.3	1.4	.8	1.4	102 (3.4)

Subject C.

	L'gth.H.	L'gth.V.	Str.H.	Str.V.	D.H.	D.V.	Angle
IV	18.9 (2.3)	17.4 (2.6)	1.4	3.2	2.	2.5	90 (8.)
IVa	16.5 (2.7)	22. (3.)	2.4	2.9	1.8	3.9	101 (9.)
IVaRH	18.5 (2.2)	18.3 (1.4)	1.3	2.	2.4	2.4	90 (1.)

Subject F.

	L'gth.H.	L'gth.V.	Str.H.	Str.V.	D.H.	D.V.	Angle
IV	20.8 (1.6)	18.6 (2.1)	3.4	2.5	2.6	2.3	94 (6.)
IVa	15.8 (2.7)	15.3 (2.9)	3.	2.4	2.	3.3	107 (6.6)
IVaRH	14.1 (1.9)	15.5 (1.8)	1.	2.	1.	1.	90 (3.7)

Subject R.

	L'gth.H.	L'gth.V.	Str.H.2	Str.V.	D.H.	D.V.	Angle
IV	16.3 (2.5)	15.2 (1.6)	2.	2.7	1.7	2.	91 (4.5)
IVa	17.5 (2.9)	17.5 (3.6)	1.9	3.1	4.6	2.9	85 (7.5)
IVaRH	18. (2.)	20.8 (2.9)	1.9	2.2	2.8	4.	90 (3.)

This table shows that the addition of the right hand in the process improves slightly the accuracy with which the subject makes the drawing with his left hand as far as the equality of the lines is concerned, except in the case of C. With all the subjects, however, the errors in straightness, direction, and angle are greater when the right hand draws with the left than when the left hand draws alone. The drawing made with the right hand is in general more accurate than either of the drawings made with the left.

Table XII gives the accuracy records for the second model. Under "L'gth. 1" and "L'gth. 2" are given the average lengths of the first and second lines. The straightness values for the two lines and the average measurement of the angle follow in the other three columns. Here it will be observed that the addition of the right hand does not have as much effect upon the accuracy of the left-hand drawing as far as the equality in length of the lines is concerned. The straightness of the lines, however, is less in every case when the right hand is drawing with the left. The exactness with which the angle is approximated is not much affected.

TABLE XII

Accuracy (Second model)

Subject A.

	L'gth.1.	L'gth.2.	Str.1.	Str.2.	Angle
IV	19. (1.8)	15. (1.4)	2.8	2.6	94.6 (4.8)
IVa	17.3 (1.8)	13.4 (1.2)	3.5	2.2	94. (7.)
IVaRH	14.5 (1.)	14.7 (2.2)	2.	1.	101. (6.4)

Subject C.

	L'gth.1.	L'gth.2.	Str.1.	Str.2.	Angle
IV	19.3 (3.9)	23. (3.1)	1.9	1.6	88. (6.9)
Va	18.3 (1.9)	20.9 (2.7)	2.7	1.2	90. (5.)
IVaRH	14.9 (1.1)	16.1 (1.8)	2.3	1.3	95. (4.)

Subject F.

	L'gth.1.	L'gth.2.	Str.1.	Str.2.	Angle
IV	27.6 (3.)	23.6 (2.8)	2.5	1.3	94. (6.8)
IVa	24.4 (2.2)	22.4 (2.4)	5.1	4.4	84. (6.)
IVaRH	19.7 (3.4)	15. (2.5)	1.7	1.7	103. (6.)

Subject R.

	L'gth.1.	L'gth.2.	Str.1.	Str.2.	Angle
IV	21. (1.8)	20.6 (1.9)	2.6	3.3	100. (10.)
Va	24.4 (2.8)	22.6 (3.5)	3.9	3.5	82. (9.6)
IVaRH	22. (1.2)	21.9 (2.3)	2.8	1.9	93. (6.6)

The averages for the time records of the drawings made from the first model are given in Table XIII. This table gives under "Horiz." the time for the horizontal line, under "Vert." the time for the vertical line, and under "Total" the time for the whole drawing. The figures in parentheses are mean variations.

TABLE XIII

Time (First model)

Subject A.

	Horiz.	Vert.	Total
IV	3.46 (.33)	3.02 (.52)	6.48 (.72)
IVa	3.60 (.25)	3.26 (.43)	6.86 (.49)

Subject C.

	Horiz.	Vert.	Total
IV	3.99 (.74)	3.46 (.66)	7.37 (1.35)
IVa	3.92 (.49)	2.60 (.48)	6.52 (1.08)

Subject F.

	Horiz.	Vert.	Total
IV	4.02 (.65)	3.52 (.43)	7.54 (1.07)
IVa	4.44 (.68)	3.81 (.56)	8.25 (1.13)

Subject R.

	Horiz.	Vert.	Total
IV	2.42(.30)	2.03(.27)	4.45(.52)
IVa	3.14(.28)	2.79(.37)	5.93(.59)

This table shows that with all the subjects except C. the addition of the right hand results in slower time. With C. it results in a faster time. The time records for the second model are given in Table XIV.

TABLE XIV

Time (Second model)

Subject A.

	1.	Point	2.	Total
IV	3.26	.50	2.61	6.37
IVa	3.55	.49	2.77	6.81

Subject C.

	1.	Point	2.	Total
IV	3.67	1.47	3.34	8.48
IVa	4.00	1.12	3.25	8.37

Subject F.

	1.	Point	2.	Total
IV	3.81	.95	3.64	8.40
IVa	4.82	.69	4.54	10.05

Subject R.

	1.	Point	2.	Total
IV	2.22	.59	2.29	5.10
IVa	2.78	.49	2.42	5.69

This table shows that the addition of the right hand gives a slower rate of drawing for all subjects except C. With him the rate is slightly faster again.

The introspections, in so far as they have to do with the relative certainty with which the figures are drawn under the two different conditions, give a wholly unexpected result. It appears from them that the addition of the right hand does not increase the subject's certainty with regard to the left-hand drawing, but decreases it instead. The introspections, however, furnish the explanation for this result. All the subjects reported that their attention was for the most part upon the left-hand drawing, but that it would shift over at times to the right-hand one. This

would occur several times in the course of the drawing of a given figure. It occurred most often in the case of F., who reported that attention oscillated between the two drawings as many as seven or eight times in the course of the process. This shifting of the attention occurred least often in the case of C. He was the only subject who seemed able to keep his attention upon both drawings at once. The time results show that C. was the only subject for whom the addition of the right hand did not result in longer drawing times.

The pressure records may be briefly dealt with. They do not show a different form of curve for each series. The typical form of the record of pressure changes is the same for the subject's left hand whether it is drawing alone or with the right hand drawing at the same time. Thus we have two series, one more certain than the other, yielding pressure curves which do not reflect this difference in attitude. The accuracy records of the two series are little different. The time records do show a difference between the series, for in the case of all the subjects except C. the time is somewhat longer for the less certain series. This, however, by itself is not very significant, and for the explanation of these results we must turn to the introspections already mentioned. The addition of the right hand in the drawing process, changing as it does the subject's total reaction, brings about a change in attitude too, and this change is experienced introspectively as a dividing of the attention, or a shift of the attention from the left hand. Thus, although the objective form of the left hand's reaction remains the same (except in its time characteristics), the change in the subject's total reaction has brought about a change in attitude. Introspectively this is designated as distraction of the attention, and it is natural that a slower rate of drawing should be found to accompany it. The case of C. is worthy of special comment. Of the four subjects he was least affected with regard to his attitude of certainty by the addition of the right hand. He is also the subject who has shown himself throughout to depend most upon kinaesthetic sensations and images. He seems to have had less difficulty than any of the others in adopting the new form of symmetrical draw-

ing. His time records show that instead of lengthening the time of the drawing, as is the case with the other subjects, the addition of the right hand actually shortened it a little. It appears, then, that the subject who makes greatest use of kinaesthetic factors is least affected by the change in the form of the total reaction made by the addition of the right hand in the drawing process.

Summary and Conclusion

The purpose of our investigation was to see whether there was a correlation between the conscious attitudes experienced by the subject in connection with his reaction to certain instructions, and the objective form of the reaction as measured by its accuracy, rate, and pressure. We now summarize briefly the results of the experiments.

In the first place, the practice series yields nothing of final value except the indication that the attitude of certainty in some given part of the process may be correlated with a slightly faster rate of drawing.

Series II, IIa and IIb, in which each subject was given a different task, yield results which differ in their definiteness, some of the figures being better suited to the purposes of the experiment than others. With A., who drew the two series of circles, the first anti-clockwise and the second clockwise, the introspections showed that the second halves of the circles were always less certain than the first halves. The time records show that the rate of drawing increases during the first half and decreases during the second half. The pressure records show that in both series the pressure of the first half rises gradually and regularly with never a decrease, while in the second halves this regular increase is not continued, irregularities occur, and sometimes a decrease in pressure appears. In the anti-clockwise circles a point of maximum uncertainty was located in six of the experiments. This point always came in the third, or between the third and fourth quarters of the circle. The time records show that when it is thus located it comes at the point at which the greatest retardation in rate is taking place, and the pressure records show that at this point too there is a decrease in the amount of pres-

sure. Thus, in this series for this subject certainty seems to be correlated with an increasing rate of drawing and steadily increasing pressure; uncertainty with a decreasing rate and irregular pressure characteristics.

The triangle figure used in Series II and IIa with C. proved to be very unsatisfactory. The results did, however, give some indication that the attitude of certainty is accompanied by a more regular pressure curve than the attitude of uncertainty. Where the figure ended in a downward stroke (Series IIa) an attitude of certainty was sometimes reported during the drawing of the last line. When so reported the pressure curve of the drawing was found to end in a "plateau."

With F., whose task in Series II and IIa was the drawing of four slant lines combined in two different ways, it is found that in general certainty goes with a longer line and a faster rate of drawing than does uncertainty, and the lines which are mechanically easy for the subject to execute are usually the lines drawn in this way. This, however, does not necessarily hold throughout, because the first line in each drawing is the most certain, although in the first drawing it is mechanically easy and in the second mechanically difficult. This certainty is correlated with a definite form of pressure curve which is followed in the drawing of the first line throughout, and is departed from only in the two cases in which this first line is called uncertain. In the other lines marked contrasts in subjective certainty are reflected in differences in the pressure curves, but small differences are not so reflected.

The most satisfactory figure in Series II and IIa proved to be the four vertical lines which formed the task for R. He was usually able in his introspective report to rank the lines in their order of certainty, and this gave a good basis of comparison with the records of accuracy, time, and pressure. The most certain and least certain lines of each experiment were compared for accuracy and time. It was found that there is practically no difference in accuracy between the most certain and the least certain lines, but the most certain lines are drawn at a somewhat faster rate than the least certain. As for pressure, it is found

that there is a very definite form of curve which always goes with certainty, and a variation from that form of curve always goes with uncertainty, this variation being greater or less according as the degree of uncertainty is greater or less. In most cases also the curve for the most certain line is between the other two in height, this holding in 80% of the cases. These results hold both in Series II, in which the lines were drawn downward, and in Series IIa, in which they were drawn upward. Thus, in this series with this subject we find that the accuracy of the drawing has no effect upon the attitude experienced in connection with it. The time, however, is less for the certain than for the uncertain lines, and a definite form of pressure curve is found to go with certainty, and some variation from this form with uncertainty.

In Series III, in which the task was drawing down to a point, the special instructions as to the rate of drawing were given. The subjects are differently affected by these instructions. A. is least affected, and as his pressure records are unsatisfactory very little can be drawn from his results, except that the introspections show that an attitude of certainty is always accompanied by a clear visual image. C. is most affected by the special instructions, the instruction to draw fast resulting in a large increase, and the instruction to draw slowly, resulting in a large decrease in the number of attitudes of certainty reported. The pressure records throughout for this subject show a correlation between certainty and a definite type of curve. With F. no noticeable change in the number of attitudes of certainty is brought about by the instruction to draw at a slow rate, but with the instruction to draw at a fast rate the number of attitudes of certainty is increased. This subject draws at a slower rate naturally than any of the others as shown by his records throughout. With this subject no correlation was found between certainty and a definite type of pressure curve in the series in which there were no special instructions and in the series in which the instruction was to draw at natural speed; but in the series in which the special instructions were to draw at a slow or at a fast rate, this correlation was found. With this subject in Series II and IIa it was found that large differences in subjective certainty were reflected

in the pressure records, but that for small differences no corresponding objective differences could be determined. So also here, it seems that the instructions "slow" and "fast," increasing as they do the subjective differences in attitude, make the objective changes in pressure great enough to be recognized. This subject drew with greater pressure than any of the others, and seemed throughout less sensitive than they to small differences in pressure. The results of this series with R. are quite analogous to those already obtained with him in the previous series. Certainty is found to go with a long line, a fast rate of drawing, and a definite type of pressure curve, and as before with him the accuracy of drawing seems to have no effect upon his certainty. The individual differences exhibited by the subjects in this series are interesting in that they seem to show that persons of different imaginal type are affected in different degrees by the same special instructions.

Series IV gave an unexpected result. It was thought that when the right hand was added in the drawing process greater certainty in the drawing made with the left hand would be the result of the better motor control afforded by the symmetrical movement. It was found that the reverse was the case, the subject reporting less certainty when the right hand drew with the left than when the left hand drew alone. The objective records show that there is little difference in accuracy, and no definite difference in the pressure characteristics between the two series of left-hand drawings. The time records, however, show that the drawings made by the left hand when the right hand is drawing symmetrically with it are made slightly more slowly than when the left hand draws alone, except in the case of C. Thus we find that the only difference in the objective records made by the change in the total form of the reaction which comes with the addition of the right hand is a slight increase in time in the case of three of the four subjects. Nevertheless, the introspective reports betray that with this change in the total form of the reaction there comes a change in attitude in the direction of less certainty. This is described subjectively as a shifting of the attention from one drawing, or one part of the reaction, to the

other. Thus, with a change in the total reaction we find a change in attitude. Subject C., who was least affected as to his attitude by the change, is also the only subject whose time is not lengthened, and he is the subject who throughout the investigation has been found to use chiefly kinaesthetic imagery. Here again, therefore, we find an instance of imaginal type characterizing the degree of change of attitude under special conditions.

The general result of our investigation is positive. In spite of numerous exceptions and some incomplete records, the results are sufficiently uniform and definite to show that under the conditions of our experiment some objective characteristics of the subject's reaction vary with his introspective attitude. The accuracy of the drawing seems less important than the other characteristics for the appearance of certainty. In some cases greater accuracy and certainty go together, but more often with the same subjects they do not. As for the time factor, certainty is usually found to go with a faster, and especially with an accelerating rate of drawing. Certainty is found also in most cases to be correlated with a definite type of pressure curve, and uncertainty with a variation from this type. In some cases, to be sure, notably with subject F., this correlation does not hold between small differences in attitude, but since it holds between large differences, this lack of complete correlation may be attributed to the crudeness of the apparatus which is not sensitive enough to show slight objective changes. In Series IV and IVa we find no appreciable change in the pressure curves of the left-hand drawings —the form of the left hand's reaction is the same in both cases. In this series the difference in subjective reaction is, however, correlated with the difference in the subject's total reaction which is brought about by the introduction of his right hand into the process.

The objective characteristics that we have measured with our apparatus, though they are directly involved in carrying out the instructions, are by no means the only ones that might vary with the changes in subjective attitude. Unconscious eye-movements, incipient movements of the limbs (especially of the arm and hand not used in the drawing), innervations of the larynx, etc.,

might all be part of the subject's reaction to the instructions and might vary with his attitude, just as we have been able to show that certain characteristics of the movements most obviously involved in his reaction vary with changes in attitude. Since it is possible that any of these other motor processes might have been as closely and perhaps more significantly correlated with the attitudes of certainty and uncertainty, the writer hesitates to claim anything final for his results. The investigation is regarded rather as a tentative, exploratory one. The results in his estimation do demonstrate that the conscious attitude is accompanied by relevant objective differences in bodily reaction, and he ventures to express the hope that other investigators will be encouraged to follow up a line of investigation which appeals to him a promising one.

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COMPLEX REACTIONS OF THE DOG: A PRELIMINARY STUDY[1]

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This study, originally begun with the hope of approaching the number processes (counting, estimating, or reacting to number) began with an attempt to set up rhythms of action on the part of the dogs. Its scope as here reported covers the preliminary stages in the process of learning, and became, necessarily, a study of methods. The apparatus used can be manipulated from outside the experimental room, and is fully described. The training series is described in a way which has not heretofore been done, and is advocated as an equalizer of animals which inevitably differ in a large number of respects. Certain observations regarding the transfer of habits learned in one series, and of the retention, seem important enough to present at this time. The method of treating the data of the experiment has also some novel features and leads to the conclusion that the evidence for discrimination is to be looked for in the integration of acts, which demands a special display of results, rather than a table of summary averages of accuracy.

Two pertinent criticisms have come from the Chicago laboratory with regard to current studies of discrimination in animals. One deals with the method of treatment of data, the other with the nature of the stimuli. Hicks and Carr (3) have shown that the percentage method of tabulation of results of learning does not accurately nor adequately represent the activities of the animals under investigation. It follows that a percentage of error curve is, for psychological purposes, an uncertain datum for inferences. And Miss Weidensall (10) has shown that when a black and a white surface are presented to an animal (for discrimination), the white surface is of greater importance in modifying the behavior than is the black, and that what appears to be a discrimination may prove to be a "simple recognition." If these two criticisms are accepted, it is clear that the evidence

[1] I wish to acknowledge with pleasure my indebtedness to Dr. J. M. Flint of the Department of Surgery in the Yale School of Medicine, for the use of dogs and quarters in which to carry on this experiment.

for discrimination in animals is seriously undermined; also that the treatment of the results of behavior must be revised if discrimination, in the psychological sense, is to be demonstrated.

With a view to contributing to this question, the present preliminary study was undertaken as an approach to a fuller investigation of more complex processes of intelligence, especially the so-called "number" or "counting" processes. The experiment has failed to reach a point at which any contribution can be made to the latter problem, and this report presents only the behavior in the beginning stages of learning.

It is possible to exhibit behavior of animals in different ways. If the experimenter is interested in a statistical problem, percentages of error, averages, means, standard deviations, etc., are important, and from them certain conclusions can be drawn regarding the adaptation of animals to a given situation. But the successes and failures, the right and wrong choices, summarized in a percentage, conceal rather than exhibit the actual behavior of an animal in response to a controlled stimulus, and encourage generalized statements regarding groups of animals. In analyzing specific varieties of behavior, therefore, such as that involved in discrimination, this concealed behavior must be taken account of. And since the effort is to discover whether a certain type of internal behavior accompanies the external manifestations of choice, it would appear that a mere "Success or Failure" summary of external manifestations must inevitably fail to yield the desired data. Conclusions as to animal discrimination have therefore been mainly negative and, when positive conclusions were drawn, reviewers have frequently suggested a faulty technique.

But if an adequate description of the actual behavior were given, certain positive data would be available as to the processes of the animals. Hence, more complete descriptions rather than the experimenter's summary conclusions are needed in order that the science of behavior may progress by cumulative results. Not "do animals or these animals discriminate?" but "what processes are now going on in this animal?" is the pertinent question. And only complete descriptions, in other than percentage terms, will

yield the desired data. The present report, therefore, is concerned with the behavior, and the analysis of behavior, of dogs in learning a visual discrimination. The rate of progress of the learning suggests a slow integration of various acts of the behavior, and the presentation emphasizes this by showing the relation of the choices to an integrated rhythmic activity.

Historical

Johnson (6) has reviewed critically the experimental work of Kalischer, Munk, Rothmann, Swift, and others. In his own dogs, he was able to discover no conclusive evidence of discrimination of pitches, but succeeded in setting up an association between food and noises, in which the localization of the noises apparently controlled the reactions. E. M. Smith (7) found his dogs able to make color discriminations "weak, unstable and easily inhibited by differences of luminosity." Colvin and Burford (1) found that changes of form inhibit color discrimination in dogs. Hunter (5) succeeded in setting up a visual discrimination (?) in the case of two dogs to the extent of 72% and 60% accuracy out of 560 and 650 trials. The experiment was characterized by "helplessness on the part of the dogs" and a "lack of resourcefulness." The position of the dog's head at the moment of release seemed to determine the direction of movement. Successive choices tended to encourage the maintenance of an orientation motor attitude for periods of increasing length. Franken (2) had earlier characterized similar reactions as "sensory thought processes." The use of this rubric is justified by Franken from the fact that the reaction is the release of an expectant attitude—as contrasted with reactions which are immediate (p. 51). If the animal possesses expectant attention, it possesses the necessary rudimentary organization of intelligence for thought processes.

The Dogs

Six dogs, three male and three female, were used. The animals are referred to as BTF (Black Terrier, female); BF (Black, female); BrF (Brown, female); BM (Black, male); BBM (Brown Bull, male); WM (White, male). These dogs were

mongrels and had been confined to the kennels for varying periods. The precise ages and experience of the dogs were unknown. In the kennel the males were fighters; the females were smaller, younger and more docile.

The use of dogs picked up from the street has been criticized by many authors. The chief objection seems to be that on account of a lack of knowledge regarding the previous experience of the animals, it may be that the behavior observed in the experiment is simply a repetition of behavior already learned elsewhere, and that the experimental conditions do not control the action, and particularly the learning. But the same objection may be urged against two dogs which have been reared in the laboratory. There is no typical dog. Each one shows marked individual differences. It seems desirable, therefore, to devise a method which shall equalize these differences to some degree, and bring the animals to a common basis for comparison. Not only is this necessary in order that the grouping of results from a single experiment may be valid; but it is the only means by which various pieces of work may be adequately compared. So far as is known this equalizing has not heretofore been done systematically. Vague references to a training series leave one in the dark as to the steps employed. In the present experiment a simple alternating habit was developed as a basis of comparison, and the growth of this rhythmical habit is fully described. And while this method is believed to equalize the dogs upon a level for a psychological experiment, we have preferred to hold these results as tentative until the method can be used upon young dogs.

Notes on Behavior in Learning the Alternating Habit

In view of criticisms relative to the presence of an experimenter or master in the vicinity of the dog while choosing, and to rule out the possibility of unconscious signs from which the dogs might take the cue, it was so arranged that from the beginning of experimental work proper (after preliminary training), the experimenter was absent from the room in which the reactions and choices took place. This necessitated a training series which

was carefully watched. For this purpose Apparatus A, a modified form of the Yerkes light discrimination box, was used (Fig. 1). The apparatus was reduced to two parallel alleys open at one end. During the first month the animals were trained by regularly progressive steps to pass alternately from one alley to the other, securing in each a piece of meat.

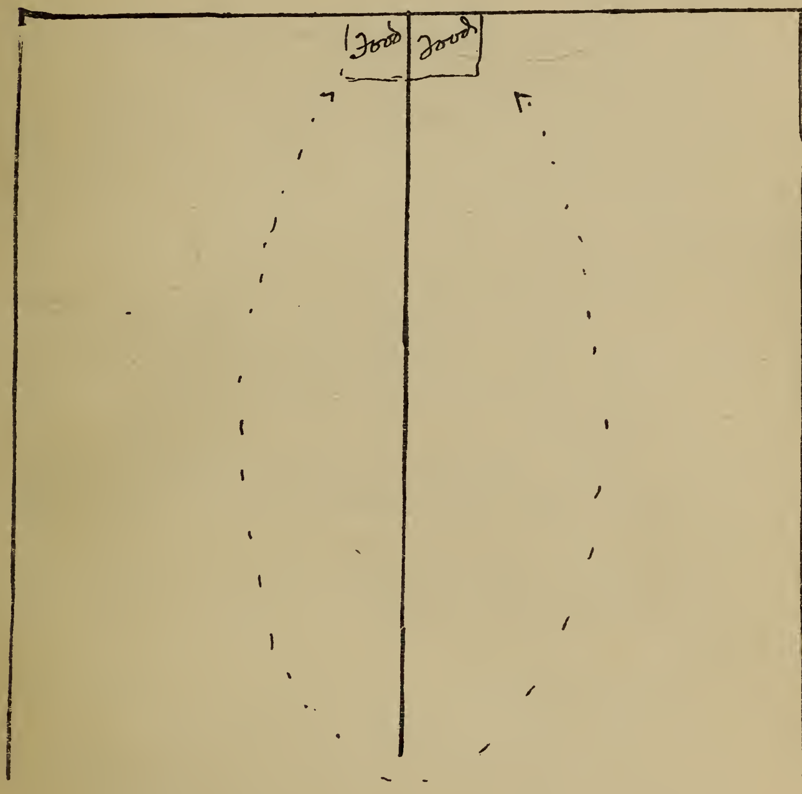


Fig. 1.—Apparatus A

Nov. 20, 1914. Each dog was taken to the apparatus room in turn, and fed at the food box in each alley of the apparatus. All the animals betrayed signs of awareness of the presence of food, three coming again and again to the vicinity of the meat, standing rigidly and with lips wrinkling. Each dog was permitted to investigate the room and apparatus so far as he would.

When the lights were extinguished, the dogs crouched close to the experimenter's feet. All tended to follow the experimenter's movments, but in different degree of proximity. BF, BM, and WM gave evidences of fright. It was found necessary to drag them into the apparatus and up to the food box, and immediately after snatching the meat, they would back away to the room outside the apparatus.

Nov. 21. Same. Less appearance of fright.

Nov. 22. WM still showed signs of fear and had to be dragged to the food box. All others stood quietly by the food box.

Nov. 23. With a piece of meat held just before the dog's nose, the dog was led to and fro about the room, and finally into the apparatus, being fed only at the food box. WM, this day, seemed less fearful, but it was still necessary to pull him to the apparatus occasionally.

Nov. 24. Same. BrF, introduced to the experiment at this point (to take the place of another dog which had died), showed no signs of fear. The experimenter took his stand in the center of the apparatus and led the dogs from alley to alley.

Nov. 25. Same.

Nov. 26. Same. Dogs now ran around behind the experimenter from one alley into the opposite alley without much coaxing.

Nov. 27. Same. Dogs would start to run to the opposite side if anything was dropped on the floor. They apparently waited for a cue.

Nov. 28 to Dec. 3. Same. If meat was not in evidence when the dog arrived, he would scratch at the trap door.

Dec. 4. Food loaded at opposite ends of alternate shelves of the chute (Fig. 2), so as to drop first in one alley, then in the opposite. The experimenter took his stand outside the apparatus without disturbing the reactions.

Dec. 5. Same. WM gave no evidence of fear.

Dec. 6-10. Same. Dogs proceeded rhythmically from one alley to the other.

Dec. 11. Experimenter took his stand at N, the doorway of the room. BrF, WM, BF, BTF each stopped several times to

look at the experimenter, but proceeded without overt encouragement.

Dec. 12-16. Same.

Dec. 17. During this day's run the experimenter backed still further out of the room without inhibiting the dogs' behavior.

Dec. 18-20. The experimenter did not advance into the room during these three days. During parts of the run, he stepped out of sight, being guided in the operation of the apparatus by the sounds of the dogs' movements.

It will appear from these notes that one month was consumed in setting up an alternating habit which would run its course during each day regardless of the presence of the experimenter. Whatever cues may have set off the adjustment, they did not come from the experimenter. In bright light, as also in a dim light which just permitted the dog to be seen, a pendular-like swing was set up, with a short stop at each end of the swing, and continued until no more food was found.

No systematic tests were made as to the length of time the rhythm persisted after the failure to secure food. Since the swings did continue, however, the cue to the continuation of the rhythm was not invariably the just prior "act of securing the food." Sight, sound and odor of food about to be secured likewise were excluded. But sights and odors, so far as they referred to the total situation, were not excluded, and it is quite possible that these, together with the kinaesthetic and organic qualities involved in approaching the position at which the food is usually obtained, are to be looked to for explanation of this behavior. To make a general test of the effect of the presence of a larger amount of odor, the front of the chute was removed so that all the meat except that on the four lower shelves was exposed. No effect could be observed in the dogs' behavior.

The experiment with controlled stimuli begins, then, at this point (with an unknown number of influential factors) and should proceed analytically in the control and variation of each. This is of course similar to the case of any human or other animal experiment. In the human being, a complex set of fairly well automatized language habits takes the place of this simple alter-

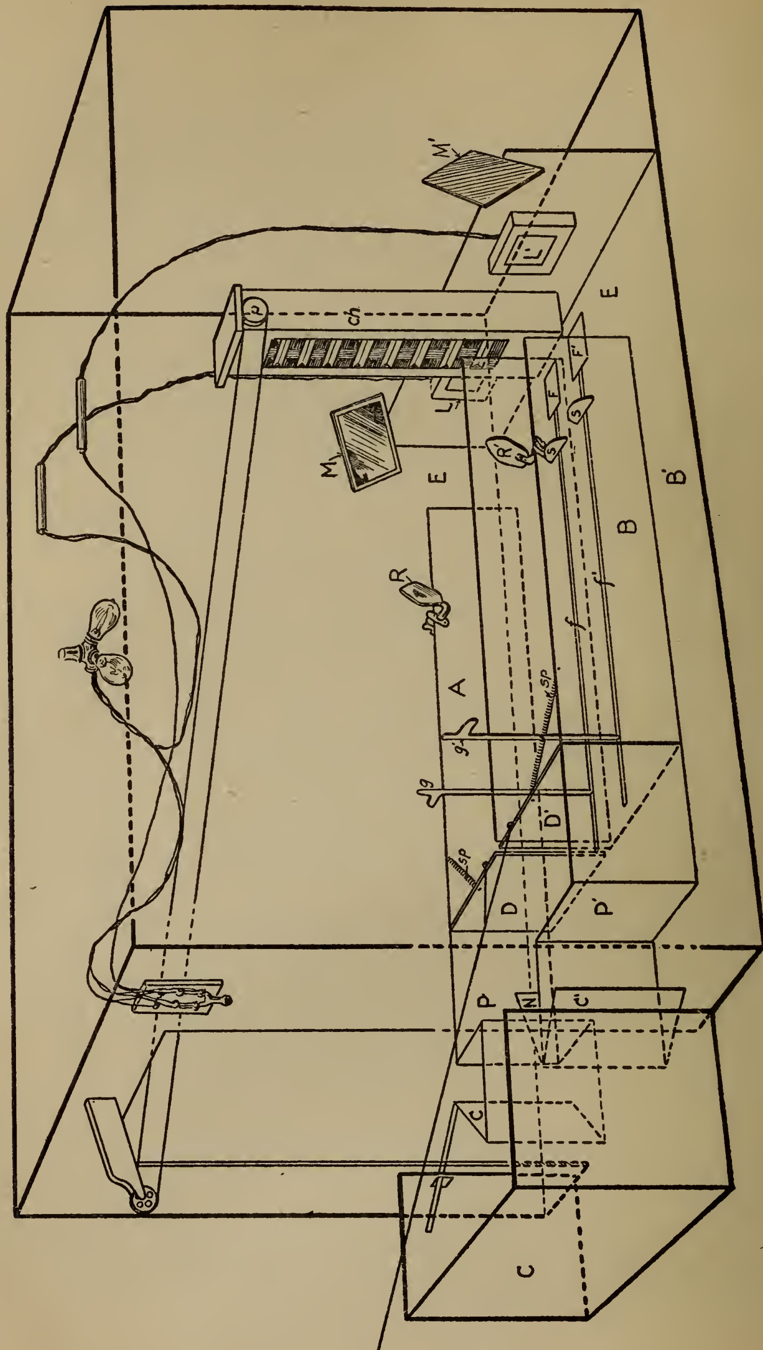


Fig. 2, Apparatus B.

nating habit, and by the variations of the verbal expressions the experimenter infers the experience of his human subject. The alternating habit of the dogs is far less complex but has the same function, so that in the present experiment we have the advantage of a single simple adjustment, the variations of which will betray the relative influence of the stimuli used. It now becomes necessary to determine whether this simple alternating habit can be broken up or complicated by means of such stimuli, without the presence of the experimenter.

Herewith (Fig. 2) is shown a perspective drawing of the experimental room and the apparatus. The walls of the room were lined with black cloth, and the starting box (C) and the doorway were also covered so as to exclude any visual stimuli from the outside. The dog was brought to the starting box, and there confined until the apparatus was adjusted. The doors, D and D′, were then pulled back, the small door, N, releasing the animal into the vestibule through a narrow passageway which necessarily gave him a direct orientation toward the two alleys. Having passed into one of the alleys, the doors D and D′ were closed behind him, the further pathway leading through the exit, E or E′, through the return alleys, A′ or B′, through the small doors, c or c′, back to the starting box, C.

After a few days' experience, it was found no longer necessary to close the doors behind the dogs, since they proceeded along the pathway back to the starting box. The reactions therefore involved a more or less continuous run (for sixteen choices), the animals stopping only to secure the piece of meat at the foot of the chute.

In greater detail the additions to the apparatus as found in Fig. 2 may be described under the following headings, *Food Chute, Stimulus Lights and Wiring, Reflectors, Screen, Dumping Apparatus, Movable Partitions, Starting Box, Operation of Doors, Curtains.*

Food Chute: In order to drop a piece of meat at the appropriate spot in the apparatus, a chute was devised, CH. This is an enclosed box 5 ft. x 6 in. x 6 in. set upright midway between the two alleys, in an opening at the end of the mid partition. Within the box are two horizontal shafts, over which runs a broad endless belt. The belt carries 16 shelves, each 1 in. deep and 5 in. long, set so that each end hangs over the corresponding alley. The shelves are 3 in. apart upon the belt. Attached to the outside end of the

upper shaft is a pulley and over this pulley runs a cord to another pulley outside the experimental room. When the cord is pulled, the belt carries the shelves downward and as each one reaches and passes around the lower shaft, it deposits its contents at the foot of the chute. Before the dog was brought to the experimental room, the chute was loaded, by placing sixteen pieces of meat (rolled in cornstarch so that they would slip) upon the shelves in such a way as to drop at F and at F′ in an order determined on for that day, or series. The experimenter may thus stand outside the experimental room and while the dog is returning to the starting-box, drop another piece of meat in the same or the opposite alley and be ready for the succeeding reaction.

Reflectors. Two mirrors, at M and M′, were set to reflect an image of F and F′ to a hole in the wall at the experimenter's station. From this station the experimenter therefore could observe any failure of the apparatus. To intensify the brightness of the image of F and F′, small mirrors were set at R and R′, at the top of the side partitions of the alleys A and B. These reflect a beam of light from the stimulus lights to the foot of the Food Chute.

The Stimulus Lights. A row of five sockets for 110 v. finger size heat lamps was placed upon a support, and this support attached to the wall at the end of the alley. The lamps were wired in parallel so that one or five lamps could be used. The tops of the lights were visible through a rectangular opening, six by four inches. The number of lights used differs for the several series and is discussed in that connection. In the later series, one or more lights were shown in both alleys, and to facilitate the manipulation of the stimulus lights, a special wire was run directly from the main current to the number of sockets necessary. A double knife switch upon the wall outside of the room at the experimenter's station, controlled the remainder of the lights, so that a single throw of the switch turned out the lights in one alley, except those on the direct wire, and turned on the corresponding lights in the other alley. It is the stimulus lights, L and L′, which reflect from the small mirrors, R and R′, to the foot of the Food Chute. This serves the double purpose of giving to the experimenter a brighter image of the food position, and also enables the dog more easily to locate the piece of meat.

Screen. A small wooden screen at S and S″ likewise serves a double purpose of shielding the food from the dog until he has entered the alley far enough that the doors may be closed behind him, and also as a scrape in connection with the dumping apparatus.

The Dumping Apparatus. If the dog chooses the wrong alley, i.e., the one from which proper stimulus and food are lacking, the piece of meat must be removed before the next reaction occurs. To accomplish this conveniently, a hole through the floor at the foot of the chute, is covered with a paddle shaped trap door, F and F′. This trap door slides under the screen, S and S′, and its contents are scraped off and fall through the hole in the floor. The mechanism for operating the trap is a long arm, f and f′, which slides along the floor between guides. At g and g′ an upright arm rests in a malleable iron socket upon the long arm. The upright arm is fixed at its center to the mid partition, its upper end being free. To operate the trap,

therefore, it is only necessary to push forward upon this upright lever. Reversing the movement closes the trap again.

Movable Parts. To give the animal the best possible orientation toward the lights, as it leaves the starting box, the right angle parts, P and P′, were devised. These are pushed back out of the way when the apparatus is not in use. The doors, N, c and c′, are attached to these movable parts.

The *Starting Box,* C, is a three sided, hinged box, the open side being directed toward the apparatus. The top is covered over with cloth so that an animal within cannot see what is going on around the box. This covering also extends up over the doorway of the room during the course of the experiment. To open and close the doors, c and c′, a slender rod[2] was fastened to the top of each, and passed through holes in the wall of the room, to the experimenter's stand. The same method is used for working the levers of the dumping apparatus. The doors, D and D′, are kept closed by the springs, Sp and Sp′, and are opened by pulling upon a cord which branches at its mid point, sending one branch to each door.

No means of punishment has been used in connection with the experiment thus far. For purposes of this preliminary series, absence of punishment was decided on in order to introduce no factors which might prove to be inhibitory of the dog's reactions.

First Series

In the first series, one light was shown, at L or L′. Meat was to be found at F or F′—in the alley in which the light was shown. The overhead light was turned off so that the room was dark except for the single stimulus light. The light was shown, for successive choices, in the following order (B = Right alley, A = Left alley): B, B, A, A, B, A, A, B, A, B, B, A, B, B, A, A—16 choices, half in alley B, and half in alley A. Will the alternating habit of the preliminary series persist, or will the dog show a preference for the light or for the dark alley? The only respect in which the conditions were changed from the preceding training series was in the location and intensity of light.

During the first ten days of this experiment, the dog was held at N by a long leash extending through and hooked to the wall (instead of in a starting box, as later). When the apparatus was ready for operation, the loop in the leash was pushed from the hook, the doors, D and D′, opened so that the light from the

[2] A part of the apparatus devised by the writer in connection with some work on Color Vision in Cats in 1907 with the Watson Spectral Light Apparatus,—unpublished.

stimulus lamp shone upon the dog, which then leaped forward into the apparatus, either alley B or alley A. By means of the long leash, the dog was then returned to N for another choice. After the tenth day, the dog followed on his own account the pathway through the return alleys to the starting box, C, from which it was released by opening the door, N, further to guard against possible influence of the dog by the experimenter.

For purposes of comparison, the percentage of error curves are given (Fig. 3) for each dog, and an average curve for the group of 6 dogs. They show that on the first day all of the dogs except one (WM) were successful in more than 50% of the choices, varying between 56.25% and 68.75%, WM being successful in 37.25% of the choices. On the second day, two of the dogs were less successful, one equally, and three more successful than on the first day. On the third day, all except one were more successful than on the second day. On the fourth day, one dog ran a perfect series, four improved, one held his record of the day before. On the fifth day, two were more successful, four were less successful, etc.

These records show that the discrimination of a light from a dark alley, widely different as these seem to be, is a difficult undertaking under the present conditions. Is this difficulty due to the previously established alternating habit? The percentages conceal the evidence on this question, and another means must be had to exhibit the actual behavior of the dogs. Behaviorism has not yet devised a terminology and the difficulties are therefore enhanced.

The day's run in the present series consisted of 16 choices; and after the first choice, the ensuing behavior may be brought under two lesser types—respectively "succession" and "alternation." Having made a choice of alley A (let us say), if the dog repeats his trip to alley A after returning to the starting box, he is credited with a "succession." If, however, the dog goes on the second choice to alley B, he is credited with an "alternation." The behavior in each series may thus be stated in terms of "successions" and "alternations."

The lights were shown in an order which, if perfectly followed, would have required 6 "successions" and 9 "alternations." In

the preliminary training series, the dog had learned to make 15 "alternations" per day. On the first day of the present series the "succession" type of behavior predominated, as follows:

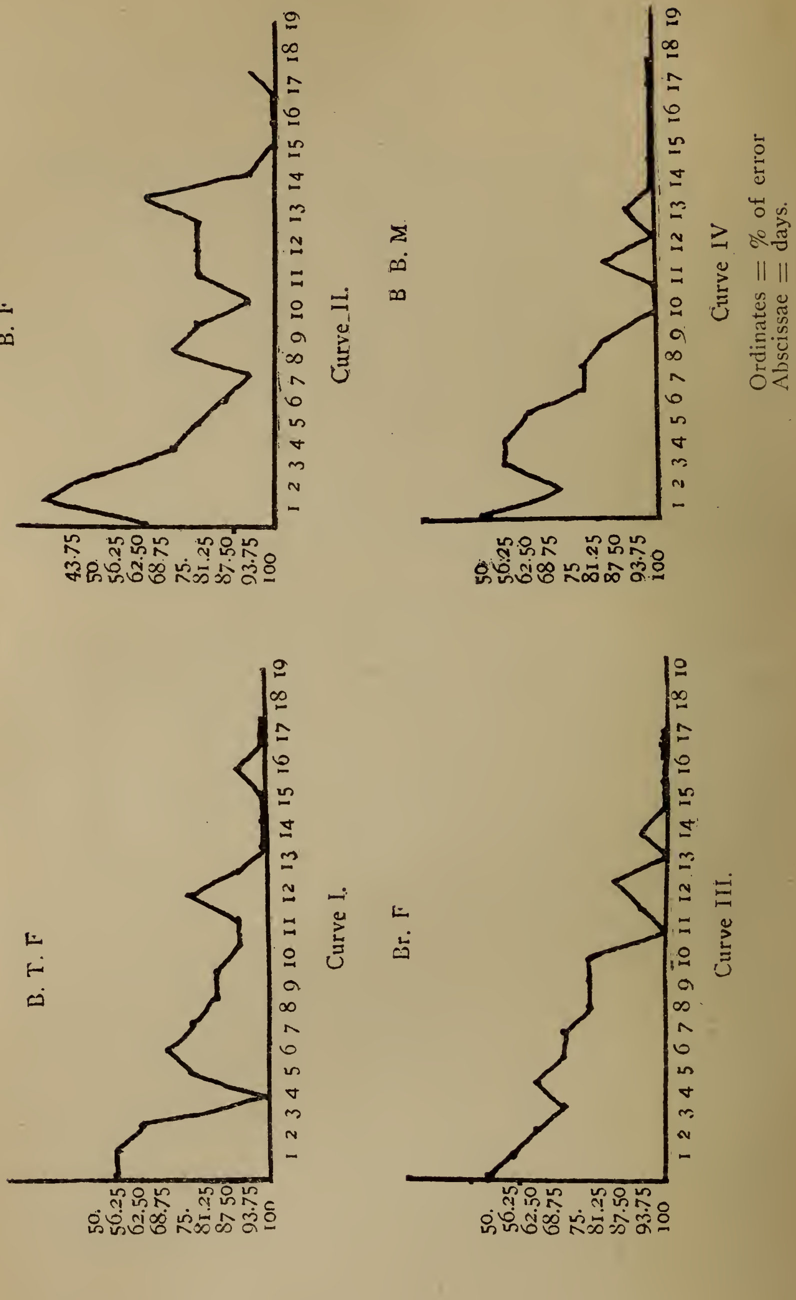
TABLE I

Dog	Behavior		No. Successful	% Successful	% of 15	Sum total %
BTF	Suc.	11	7	.636	.467	
	Alt.	4	3	.75	.20	.667
BF	Suc.	12	7	.58	.467	
	Alt.	3	3	1.00	.2	.667
BrF	Suc.	9	5	.55	.33	
	Alt.	6	3	.50	.2	.53
BBM	Suc.	5	2	.40	.13	
	Alt.	10	7	.70	.467	.60
BM	Suc.	9	5	.55	.33	
	Alt.	6	3	.50	.2	.53
WM	Suc.	8	3	.37	.2	
	Alt.	7	3	.43	.2	.40

It is at once clear by reference to the above table, that the difficulty does not lie in the persistence of the alternating habit, except in the case of BBM.

TRANSFER OF TRAINING

The first experimental series, from the human point of view, required merely a slight modification of the preliminary series. It was different from the preliminary training series by the number of "successions" required, in place of a similar number of "alternations." The "successions" were interpolated among the "alternations" in the following order—S, A, S, A, A, S, A, A, A, S, A, A, S, A, S. The change of location and intensity of light made the situation sufficiently different to inhibit the transfer of the alternating habit. Was there, however, a position error on the first day of the series, and if so, on which side? My records show, 1st, that any position error present the first day was eradicated after the third day (BTF made a perfect score on the fourth day); 2nd, that BBM, whose tendency was to continue the rhythmical habit on the first day, showed more "successions" than "alternations" on the second day; 3rd, that BF and BM developed a pronounced tendency toward "successions" on the third day. A further examination of the records shows that this last apparent tendency was a sporadic affair which persisted only for the one day.


B. T. F
Curve I.
B. F
Curve II.
Br. F
Curve III.
B B. M.
Curve IV
Ordinates = % of error
Abscissae = days.

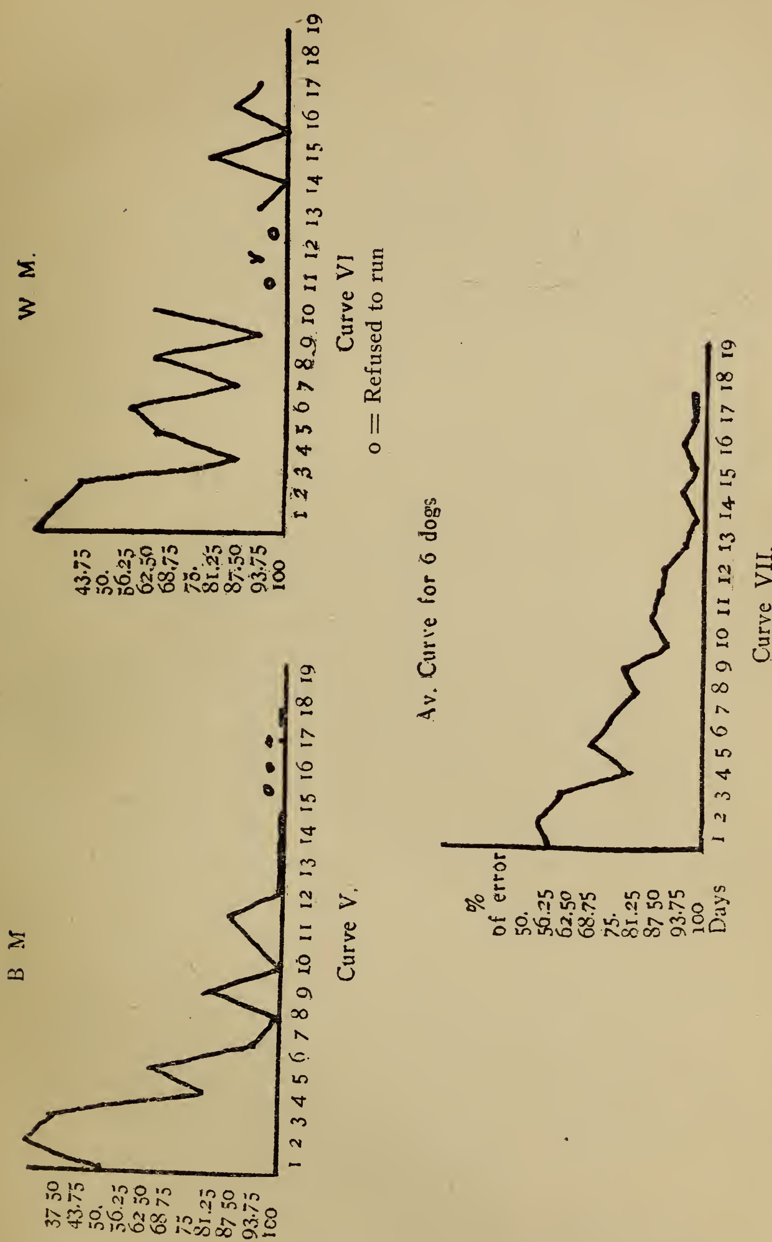

B M
Curve V.
W. M.
Curve VI
o = Refused to run
Av. Curve for 6 dogs
% of error
Days
Curve VII.

FIGURE 3

TABLE II

	Successions on L	R	Alterations L to R	R to L
		First Day		
BTF	11	0	2	2
BrF	4	5	3	3
BF	11	1	1	2
BBM	3	2	5	5
BM	0	9	3	3
WM	7	2	2	4
		Second Day		
BTF	12	1	1	1
BrF	7	2	3	3
BF	8	2	3	2
BBM	2	6	3	4
BM	3	4	4	4
WM	8	1	3	3
		Third Day		
BTF	9	0	3	3
BrF	3	4	4	4
BF	13	0	1	1
BBM	2	6	3	4
BM	14	0	1	0
WM	7	0	4	4

LEARNING TO DISCRIMINATE

In learning a series, the behavior is modified, not by success in general, but by each particular success or failure. The evidence for discrimination consists in the integration of a series of acts in a definite order (when the order of presentation of stimuli is constant) and in the variation of this order (when the order of presentation is varied). The problem here is not so much to determine whether the animal does discriminate, as to determine the particular conditions under which this modification of behavior occurs, to note how rapidly the dogs respond to the success or failure to obtain reward, and to note the type of behavior which follows each particular success or failure. The following table shows the complexity of the progress, for the dog BTF. In the first group of columns are shown the numbers of cases in which a successful choice on the left led to a successful "succession," and to a successful "alternation" to the right; also the choices successful on the right which were followed by a successful choice by "succession" or "alternation." (S = "succession"; lAr = "alternation" from left to right; rAl = "alternation" from right to left.) The second group shows the choices which were successful by means of "suc-

cessions" and "alternations," when the preceding choice was unsuccessful. The third group shows the unsuccessful behavior which followed a successful choice. The fourth group shows the figures for unsuccessful behavior when the preceding choice was unsuccessful.

TABLE III
Dog BTF

Day of Series	Successful followed by Successful				Unsuccessful followed by Successful				Successful followed by Unsuccessful				Unsuccessful followed by Unsuccessful			
	L		R		L		R		L		R		L		R	
	S	lAr	S	rAl	S	lAr	S	rAl	S	lAr	S	rAl	S	lAr	S	rAl
1	3	2	0	1	4	0	0	0	2	0	0	1	2	0	0	0
2	3	1	1	1	4	0	0	0	3	0	0	0	2	0	0	0
3	3	3	0	1	4	0	0	0	1	0	0	2	1	0	0	0
4	3	4	3	5	0	0	0	0	0	0	0	0	0	0	0	0
5	3	1	1	4	1	2	0	0	3	0	0	0	0	0	0	0
6	2	3	1	2	3	0	0	0	0	1	0	2	0	0	0	1
7	1	3	2	4	0	1	0	1	1	1	1	0	0	0	0	0
8	3	2	2	4	1	1	0	0	2	0	0	0	0	0	0	0
9	3	3	2	3	1	1	0	0	2	0	0	0	0	0	0	0
10	2	4	3	4	0	0	0	1	0	0	0	1	0	0	0	0
11	2	4	3	4	0	0	0	1	0	0	1	0	0	0	0	0
12	3	3	3	4	1	0	0	0	1	0	0	0	0	0	0	0
13	3	4	3	5	0	0	0	0	0	0	0	0	0	0	0	0
14	3	4	3	5	0	0	0	0	0	0	0	0	0	0	0	0
15	3	4	3	5	0	0	0	0	0	0	0	0	0	0	0	0
16	3	4	3	5	0	0	0	0	0	0	0	0	0	0	0	0
17	2	3	3	5	0	0	1	0	0	1	0	0	0	0	0	0
18	3	4	3	5	0	0	0	0	0	0	0	0	0	0	0	0
19	3	4	3	5	0	0	0	0	0	0	0	0	0	0	0	0
	51	60	42	72	15	5	1	3	15	3	2	6	5	0	0	1

This table shows also the progress of integration. There is a gradual concentration of the figures under the "Successful to Successful" column, and a gradual diminution of the figures in the other columns, with sporadic lapses. The table shows in some detail that this progress is accomplished by elimination of a certain number of reactions under one type of behavior, the substitution of the other type of behavior for it, and at the same time the ordering of the precise sequences according to the established order of stimulus presentation. The behavior thereupon must be considered, not merely behavior, but behavior with regard to a discriminated object, or sign.

Similar tables have been prepared for each dog, but as will appear from our summary of the problem and difficulties, there

are good reasons for omitting them at this time. The purpose of including the above table is to indicate the method of presenting the results which will show the complexity of the process of integration of behavior which is necessary as a criterion of discrimination. By means of this table the elimination of unsuccessful "successions" and "alternations" and the points at which one type is substituted for another are shown. Taken in conjunction with the following Table IV, which shows the days on which each error occurred, an exhibit of the detailed behavior is given which retains the relation of each choice to the integrated behavior. This is valuable for psychological purposes.

Was the integration of the behavior of equal difficulty throughout, or were there points in the series which required a longer time to master? The following table shows, for the same dog, BTF, that the first choice was mastered at the fourth day, and no errors were made thereafter. The same is true of the second choice. On the third choice an error occurred on the 7th day, etc.

Table IV

Choice	Dog	Days on which errors occurred	R	L	Total
1	BTF	1, 2, 3	3	0	3
2		1, 2, 3	3	0	3
3		7	0	1	1
4		17	0	1	1
5		1, 2, 3, 5, 12	5	0	5
6		10, 15	0	2	2
7		9	0	1	1
8		2, 6, 8, 9, 13	5	0	5
9		0	0	0	0
10		1, 2, 5, 7, 8	5	0	5
11		1, 2, 3, 6, 9	5	0	5
12		12	0	1	1
13		5	1	0	1
14		1, 3, 6	3	0	3
15		11	0	1	1
16		7	0	1	1
			30	8	38

The table shows further the relative difficulty of the integration of the various acts of choice. If a line were drawn connecting the final one of the days on which errors occurred, it will roughly indicate the relative difficulty of each choice in the series for the dog BTF. It also shows that notwithstanding the

alternating habit which preceded the series, a preference or position error persisted and is shown predominantly on the 5th, 8th, 10th, 11th choices in the series. Grouping by fours the 38 errors which this dog made in establishing this integration, it will be seen that the large proportion of the errors occurred in the middle of the series.

Errors on the	R	L	Total
1st 4	6	2	8
2nd 4	10	3	13
3rd 4	10	1	11
4th 4	4	2	6
			38

Another difficulty which is apparent is that of *retention.* For the first six days the third choice was properly integrated, on the seventh there was a slip at this choice. For sixteen days the appropriate type of behavior was called out on the fourth choice and during the nine days previous there had been no errors on the first four choices. On the seventeenth day, an error was made on this choice. Summing the errors for each choice, for the entire group of dogs we have the following Table V. (S = "succession, A = "alternation.")

TABLE V

Choice 1	18	errors	Choice 11	29	S
2	23	S	12	7	A
3	15	A	13	16	A
4	18	S	14	19	S
5	34	A	15	11	A
6	15	A	16	16	S
7	14	S		284	
8	18	A			
9	5	A			
10	26	A			

Of 284 errors made by the group, 136 occurred on the first and last four choices, 148 on the second and third four choices, bearing out the former statement that the middle of the series is more difficult. This may be taken as additional evidence that the alternating habit is not carried over, since in the first and last group of four, there are four "successions" and three "alternations" and here a smaller number of errors was made than in the second and third groups of four in which there were two "suc-

cessions" and six "alternations." (These figures of course include the 18 errors made upon the first choice.) "Successions" led to 119 errors, an average of 19 5-6 for each choice; and "alternations" led to 147 errors, an average of 16 1-3 for each choice.

By way of summary of the results so far, it may be said that the apparatus described here admits of maniuplation from outside a room; that dogs can be trained to work within the room without the presence of the experimenter; that a training series of the kind described is favored as an equalizer of various species of animals; that this places the animals on a basis with reference to the experiment differing only in degree from that of the human subject whose language is behavior. As a result of the first series of this experiment it is shown that the alternating habit is not transferred to the situation which, so far as the experimenter can control it, remains the same except for the position and intensity of the light; that a study of behavior demands behavior terms and the terms "succession" and "alternation" are suggested as descriptive of the behavior of the dogs in this experiment; that a position error was developed but did not persist longer than three days; that the original behavior in this series was modified by the experience of the animals and the problem is to determine the conditions under which this modification occurs. Lack of retention is shown to be a source of difficulty. A series of 16 successive choices to be made according to an established order is shown to be of unequal difficulty in its various parts. Psychological study demands a mode of presentation of results which shall avoid vague general statistical summaries and get closer to the facts and a step in this direction is indicated by the tables. And finally it is proposed to define discrimination in behaviorist terms. The series of acts of choice which constitute the day's run may be thought of as a whole or as parts. The evidence for discrimination is to be looked for in the integration of the parts into a whole, with reference to some stimulus, which has become gradually organized as the representative of a particular mode of behavior.

Test of Persistence

At the close of Series 3, to be mentioned later, the apparatus was arranged for a repetition of the above conditions to test the persistence of the association set up. Twenty-two days had elapsed. All dogs followed the light, except BrF, who went once to the side on which no light was shown. On the second day, all dogs went to the light, except BM who went once to the dark alley. The third day all choices were correct. On the fourth day, the lights were shown in a new order: B, A, B, B, B, B, B, A, A, A, B, B, A, B, A, A. All dogs correctly followed this new order. We have here a new mode of behavior not the "successions" and "alternations" learned in a certain rigid order. The behavior has become flexible and is freed from the rigidity of the simple reflex. Something is selected and this selectivity is of a type which is the product of experience and fits only under the concept of discrimination.

Second Series

The second stage of the experiment required a similar discrimination between "two-lights-with-food" in one alley as against "one-light-without-food" in the opposite alley. The order of presentation of the two lights was the same as in the first series, viz., B, B, A, A, B, A, A, B, A, B, B, A, B, B, A, A,—16 choices. The series ran for nine days without appreciable change in the level of the curve showing percentage of error. BF became frightened at her image in the mirror during the first day and was dropped from the experiment. The results show that the dogs all started in with an excess of "successions" and that the general tendency was toward an excess of "alternations." The dog, BTF, on the first day made 10 "successions" and 5 "alternations," of which 7 and 3 respectively were successful. On the ninth day she made 3 "successions" and 12 "alternations" of which 1 and 8 respectively were successful. Having been guided at this time by percentage of error curves which concealed the true integration which was slowly going on, I closed the series and began a third series.

Third Series

In the third series "four-lights-with-food" as against "one-light-without-food" were used; and the order in which these were presented was twice in succession in the right alley, then twice in succession in the left alley. During the first four days, the dogs were permitted to run into the apparatus with the four lights showing and the other alley closed. This was designed to emphasize the four lights and to break up the tendency to "alternations" which had appeared in the second series. In the third series the curve of percentage of error remained level. There was constant fluctuation as to number and order of "successions" and alternations," but these varied from day to day and were not retained as permanent members of a growing complex. Such a fluctuation perhaps represents roughly the clawing and scrambling of animals in the problem box; or of running into blind alleys in the maze. But the retention of one type of behavior and the elimination of unsuccessful behaviors failed to occur. There was a failure to pick up a cue which might become organized as the focal point of a complex of elementary acts. The series ran for ten days (160 choices for each dog). In the second and third series together (more than 300 choices) the difference in magnitude of light area (or in intensity of illumination, or in the number of lights) distinguished the two alleys. That this situation is very different for the dog, from the discrimination of a lighted from a dark alley, is at once clear. There is again a failure to transfer a habit from the first series to the following two. It is quite possible that a prolongation of the series might succeed in setting up the association, but the testing of the method was of greater concern at this time.

Final Series

In the final series, "four-lights-with-food" were shown in one alley as against "one-light-without-food" in the opposite alley. The order of presentation was the same from day to day, except that on one day the series began on the right, the following day on the left. The curve of percentage of error for WM is typical (Fig. 4). The series ran for 20 days (320 choices for each dog)

and the accuracy varies between 50% and 81.25%. The accuracy varies little from that of the preceding series in which the first choice for each day was to be made on the same side.

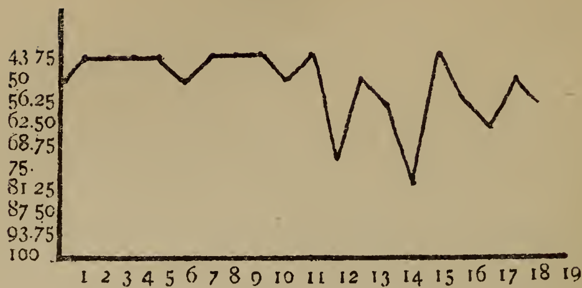
Although the accuracy of choice approaches that attained by the dogs of Hunter (5), the tables of analysis of behavior show clearly that no progress was being made and therefore there is no indication of discrimination. The following Table VI for the dog BBM shows the failure to progress in the distribution of the "successions" and "alternations" toward an expected standard; and that while there was substitution of "successions" for "alternations" and vice versa, from day to day, there was no retention of the modifications.

TABLE VI
Dog BBM

Day of Series	Successful to Successful					Successful to Unsuccessful					Unsuccessful to Successful					Unsuccessful to Unsuccessful				
	L		R			L		R			L		R			L		R		
	S	lAr	S	rAl		S	lAr	S	rAl		S	lAr	S	rAl		S	lAr	S	rAl	
1	0	0	3	4	7	0	1	2	0	3	0	4	0	0	4	0	0	0	1	1
2	0	4	1	0	5	0	0	0	4	4	0	0	1	3	4	0	2	0	0	2
3	1	3	1	1	6	1	0	1	2	4	0	1	0	3	4	0	1	0	0	1
4	0	3	0	0	3	0	0	0	4	4	0	0	0	0	4	0	4	0	0	4
5	0	3	1	1	5	0	0	1	3	4	0	1	0	3	4	0	2	0	0	2
6	0	3	0	0	3	0	0	0	4	4	0	0	0	4	4	0	4	0	0	4
7	1	1	2	1	5	2	0	3	0	5	0	3	0	2	5	0	0	0	0	0
8	0	2	0	0	2	0	0	0	4	4	1	0	0	3	4	1	4	0	0	5
9	2	4	0	0	6	1	0	0	2	3	0	0	0	4	4	0	2	0	0	2
10	0	1	1	2	4	1	1	2	1	5	0	3	0	2	5	0	1	0	0	1
11	2	2	1	2	7	0	1	0	1	2	1	1	0	1	3	1	1	0	1	3
12	0	2	0	0	2	0	0	1	3	4	0	0	0	0	4	0	4	1	0	5
13	0	1	3	2	6	0	0	3	1	4	0	2	0	1	3	0	1	1	0	2
14	0	1	2	3	6	0	1	1	1	3	0	3	0	1	4	0	1	0	1	2
15	0	2	0	0	2	0	1	1	2	4	0	1	1	3	5	0	2	1	1	4
16	0	1	2	2	5	0	1	1	2	4	0	2	1	1	4	0	1	0	1	2
17	2	3	2	3	10	0	1	0	1	2	0	1	0	1	2	0	0	0	1	1
18	0	1	1	1	3	0	0	2	2	4	0	1	0	3	4	0	3	1	0	4
19	0	2	2	2	6	0	1	1	2	4	0	2	1	1	4	0	0	0	1	1
20	0	2	2	2	6	0	0	1	2	3	0	2	0	2	4	0	2	0	0	2
	8	41	25	26		5	8	20	41		2	27	4	46		2	35	4	7	

The total percentage of errors for the final series is approximately the same as for the second and third. It is considerably more than 50%. This was an unexpected result since the change in the location of the beginning choice for each day would supposedly prevent the record from showing so high a percentage.

W. M



Curve XXIII
FIGURE 4

The only conclusion which can be drawn regarding discrimination here is that, for the conditions of this experiment, the difference between four lights and one light is too small a difference upon which to organize and control the reactions.

Attention should be called however to the fact that the reactions occurred—perhaps in response to the total situation or to some part of the situation the nature of which is unknown—in such a way as to force the recognition of the underlying organic activity of the animals. The raw material, so to speak, is present; the influence of the preliminary training series is not shown in the specific mode of behavior, yet is shown in the continuation of activity in a dark experimental room without the presence of the experimenter. Something has been carried over from the preliminary training series to the later experimental series which is not to be seen in the tables of results, but might be described as a "general set" toward experimental situation. Considering the difficulties of manufacturers to secure continued work from human beings unwatched, especially in new countries, it is worthy of note that the dogs will readily assume a "work" habit which leads them through a complicated apparatus day after day without immediate supervision.

It is hoped that this experiment may be repeated under conditions more satisfactory for final conclusions.

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AN EXPERIMENTAL STUDY OF MENTAL CAPACITIES OF SCHOOL CHILDREN, CORRELATED WITH SOCIAL STATUS[1]

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Thirty-seven children, thoroughly representative of the professional and upper middle class of England were tested and compared with thirty-one children equally representative of the lower middle class and the better class of tradesmen. There were ten tests measuring, with varying degrees of satisfactoriness, the functions of memory in its various forms; perceptual discrimination; analogical reasoning; rapidity of arm-movement; rapidity and accuracy of arm-movement under conditions demanding maximal attention; ability to divide attention or rapidly to alternate it; ability to understand spatial relations or to introduce order into one's spatial perception; and ability to comprehend conceptual relations.

In all save the tests of rapid movement, the children of the "better class" were strikingly superior.

CONTENTS

[1] The writer wishes to acknowledge his great indebtedness to Professor Wm. McDougall for constant help and encouragement throughout the progress of the investigation. Dr. Edgar Schuster helped in devising the tests and in various other ways. Miss M. Smith of Cherwell Hall, Oxford, was of great assistance in correlation work. The Headmasters of the three schools were most courteous in giving their aid. And finally, he had throughout in applying the tests the invaluable assistance of Miss M. Bickersteth.

The investigation was carried out in Oxford, but this report was written, with critical editing by Professor Angier, while the writer was serving (1914-1916) as assistant in the Yale Psychological Laboratory.

I. Aim of Investigation and Selection of Tests

What empirical differences in respect to intelligence can one find between children of different social strata? Is it possible to demonstrate that these differences are hereditary or to establish a strong presumption to that effect based on empirical evidence? The investigation here reported will, it is hoped, add a little more towards the solution of these long-mooted problems. This was constantly before us as our principal aim. At the same time it was our hope that we might also add to the rapidly growing test literature some evidence as to the nature and value of various tests, some of them new. For this reason, correlations between test and test and between tests and general intelligence were calculated.

During the spring and early summer of 1913, Dr. Schuster, Professor McDougall and the writer met frequently to devise and try out tests suitable for our purposes. Several undergraduates volunteered to act as subjects. In the fall of the same year, Miss Bickersteth and the writer tried them out on a slightly larger scale with twenty-six boys at the parish school of St. Phillip and St. James. We were thus able pretty thoroughly to sift the tests and to standardize our technique before we began in 1914 the tests below described, in the Oxford Central Elementary School and the Oxford Preparatory School.[2]

A final consideration of the suitability for our purposes of the tests chosen must be deferred until we have studied the results of each in detail. At this point it is in place, however, to state the general principles which guided us in our search for tests.

(1) Independence of the *general* conditions of school and home training. This is necessary if we are to test for mental inheritance; and the degree of this independence (which is confessedly difficult to determine) is the measure of the test's value. (But see p. 328 ff. for a fuller discussion.) This consideration led to the rejection, for example, of the "Size of Vocabulary" test,

[2] In England a "Preparatory School" trains boys from 8 to 14 for entrance into the great "Public Schools." It is, of course, a private institution, generally with boarders.

which has a high correlation with intelligence but is obviously too dependent upon home and school influences.

(2) Exact objective measurement.

(3) Reliability. A test is reliable when its result is not determined to any great degree by changing and momentary conditions.

(4) Freedom from complicated apparatus.

(5) Exact quantitative formulation. For anything like detailed comparison between two groups, it must be possible to express quantitatively in a single figure the ability of each subject in a given test. This excludes not only all tests with a simple "yes" or "no" answer, but all simple alternative tests where the grading is either "plus" or "minus," "right" or "wrong."

One or another of these criteria excludes practically every test of the Binet-Simon series. Most of these permit of no fine gradations and their dependence on accident, environment, and training is often marked.

In the following pages, tests have been given shortened and more or less cryptic names for convenience of reference. It is hoped, however, that these names give sufficient clue to the nature of the test when once explained. In the list that follows the column headed "Nature of Function Tested" must be taken as schematic and provisional. Each of these tests will be described later in detail. The ten tests, Nos. 2 to 11, are those with which we are chiefly concerned.

Name of Test	*Nature of Function Tested*
1. "Plunger"	Quickness and accuracy of movement
Alphabet Tests:	
2. "Letters" } 3. "Figures" }	Quickness of perceptual discrimination
4. "Alternating"	Quickness of perceptual discrimination and ability to distribute attention
5. "Tapping"	Rapidity of movement and ability to sustain effort
6. "Analogies"	Reasoning
7. "Dotting"	Accuracy and quickness of movement and quickness of practical judgment
8. "Spot Pattern"	Range of visual attention and ability to apprehend spatial relationships
9. "Related Memory"	Memory for words having sense connection
10. "Narrative Memory"	Memory for prose narrative
11. "Immediate Memory"	Memory for unconnected words
Discs and Circles Tests:	
12. "Disc Sorting"	Manual dexterity, accuracy of practical judgment based on peripheral vision
13. "Circle Judging"	Speed and accuracy of discrimination-judgment (perceptual)
14. "Combined Discs and Circles"	As for 12 and 13, and ability to distribute attention

II. Children Investigated

Opportunity was given us to work with two groups of subjects admirably suited for our purpose—children old enough (from 12 to 14 years) to understand and grapple with fairly difficult tasks, but young enough not to have their innate endowment too much concealed by habit. On the other hand, respective socially homogeneous groups were desired. Both these requirements we found in two Oxford schools.

The Central School is a fee-paying, elementary school, drawing its pupils from the lower middle classes. The parents are small tradesmen, artisans, and college servants—presumably on the whole neither defective nor preeminent in ability. Such a school receives practically no very poor boys and it is a fair assumption that all are well nourished. Nevertheless, several boys falling within the age limits were dropped because their obviously defective physique precluded their doing effective mental work. Those left were, therefore, a little above the average of the school in physical fitness.

The parents of the Preparatory School boys are mostly persons occupying high posts in the ecclesiastical, civil or academic world. So many of the boys are sons of Oxford "dons" that the list of names sounds like the roll of the Oxford professoriate. Such posts imply eminence in ability and culture. The entire class represented is, in England at least, the result of a long process of social and professional selection. Here if anywhere we should expect to find preeminent ability inherited. These Preparatory School boys will be called hereafter in this paper Group A and those of the Central School, Group B. These two schools are the same in which Burt (2) carried on his experiments in 1908.

Table I gives the most important figures in regard to height, weight, and age.

Table I

	Maximum	Minimum	Median	Average
	Height in inches			
Group A	63.25	56.75	58.75	59.97
Group B	64.14	52.25	57.75	57.57
	Weight in pounds			
Group A	127.5	68.5	84.	87.7
Group B	117.5	56.75	77.5	79.73
	Age in years and months			
Group A	13— 9	21— 0	12— 0	12—9.97
Group B	13—11	11— 8	13— 0	12—10.5

It will be observed that the youngest subject in Group B falls outside the limits fixed. First included by mistake, he proved so good a subject that he was retained. He was placed 12th on the tests as a whole. The omission of his marks would, therefore, slightly decrease the averages of Group B. It will also be noticed that Group A boys were slightly heavier. Probably this was an indication of somewhat superior health.

III. Application of Tests

Both Miss Bickersteth and the writer carefully practised every test before applying it to the subjects. The instructions to subjects were practically memorized by us and were kept very uniform. Great stress was laid on this point. Whenever it seemed that a subject was not doing his best, an effort was made to stimulate him to his optimum. The interest and rivalry at Group

A school rendered this seldom necessary. On the other hand, the competitive spirit even when stimulated by the offer of a prize was comparatively weak at Group B school. The boys seemed to be trying to do their best because they were told to, and to have, very often, no interest in comparative results. This whole question of interest and motive will be dealt with later. Every effort was made to put the subjects at their ease and there is no doubt that they soon became accustomed to the experimenters and the tests. The "plunger" test was retained as the first test in order to absorb initial nervousness, although it was soon obvious that the results from the test would be, for any other purpose, useless.

Unless otherwise stated, subjects were tested one at a time. According to their length, one, two or three tests were given at each sitting. From 15 to 30 minutes was the usual duration of a sitting, the latter time being never materially exceeded. In every case, the subjects were encouraged to ask questions in order to insure a proper understanding of the task. Each test was given twice to each subject. In nearly every case the writer gave it first while Miss Bickersteth assisted on the repetition. When she tested first, the writer usually gave the repetition. Since we had worked out our procedure together at St. Phillip's School, it was thought that it would make little difference which experimenter conducted the test. (But see p. 308 note, for a discussion of this point.) The interval between first and second trials varied somewhat for different tests and according to the exigencies of school work, but was kept approximately the same for the two schools. The interval will be stated in connection with the description of each test. All tests were conducted in the morning—i. e., before the mid-day meal. Any necessary time measurements were made with a stop-watch recording fifths of a second.

IV. Methods of Treating Results

Correlations.—The correlations meant are those between test and test and between tests and general intelligence. These are not only of very great intrinsic interest but they are very useful

in establishing the value of a test and in analyzing the functions tested. The subjects were arranged in ranks according to their achievements in each test and correlations were calculated by the Pearson "Product-Moment Method" adapted to ranks, the formula for which is:

$$r=2\sin\left(\frac{\Pi}{6}\left[1-\frac{6S(\nu_1-\nu_2)^2}{n(n^2-1)}\right]\right)$$

The probable error of this formula is $\frac{.7063\,(1-r^2)}{\sqrt{n-1}}$. (5)

The conception of "partial" or "multiple" correlation is one of very great importance, and one we shall use occasionally to assist us in our analysis of certain tests. A "partial" correlation is the value of a correlation between two variables, 1 and 2, for constant values of 3, 4———n. If there are 3 variables, the equation is $r_{12\cdot3}=\frac{r_{12}-r_{13}r_{23}}{\sqrt{1-r_{13}^2}\sqrt{1-r_{23}^2}}$ where the subscripts indicate the variables correlated. The subscripts 1 and 2 preceding the point in $r_{12.3}$ are the variables whose correlation is sought for a constant value of the variable following the point. (1, p. 63.)

"Reliability" coefficients are simply the correlations between two applications of one test. Brown (1, p. 86) suggests that they be called the "coefficients of *individual* correlation." They indicate the tendency of the individuals comprising the group to deviate from their own (individual) hypothetical mean. Hence they measure to a large extent the interference of those accidental disturbing factors which cause deviation from this hypothetical mean.

All our correlations are what Spearman (8) calls "uncorrected" coefficients. For while the need of "correction" is recognized, it cannot be said that Professor Spearman's formula for correction has established itself (cf. Brown, 1, p. 83 ff.).

We shall later have to try to account for the differences shown in the relative correlations between tests at the two schools—a test may show a fairly high correlation with a certain other at one school, practically none at the other school. To anticipate somewhat, one may say here that we are not always successful in this attempt at explanation. It is well to bear in mind, how-

ever, that a task may test one function in one person, another function in another. Especially is this true if the persons be of markedly different levels of intelligence. Simpson (7), for example, found very different correlations between tests according to whether he took the "good" group (university professors and graduate students) or the "poor" group (men at a Salvation Army Industrial Rescue Home). It is not surprising therefore if the correlations are not always the same in the two groups.

Measures of general tendency.—The arithmetical mean or average is computed in each group for each test, for its repetition, and for the amalgamation of first and second testings. The median is also indicated in each of these cases, together with best and worst individual achievements. The diagrams accompanying each test require some explanation. Each group was broken up into six divisions or classes, based on proficiency in that test.[3] The average of each division was then taken. This gave the value which determined the position of each class along the axis of the ordinate. Along the abscissae the classes were arranged in order from "worst" to "best." The line connecting the points thus determined is a broken ascending or descending line. With both groups plotted on the same diagram, it is possible to compare the best with the best, the worst with the worst, thus avoiding a familiar error. The angle each segment of the line makes with the abscissa measures the amount of improvement from division to division.[4] To save space the abscissa is drawn through the mean for all subjects, both groups, and not through the zero, which is a point of no significance in this connection. (See, for illustrative curve, Fig. II.)

Averages based on such a small number of subjects as compose these divisions are usually of slight value on account of large "sampling" errors. The members of these divisions were, however, carefully selected according to rank and with such homogeneous groups, the mean is doubtless an adequate measure of tendency.

[3] To make the groups exactly divisible by six, the median was in each case excluded.

[4] The scale is not the same for all the tests, however. Each test can be studied only by itself. In all figures, the dotted line represents Group A, the solid, Group B.

In the regular formula for the standard deviation from the mean (S. D. or $\sigma = \frac{\sqrt{\Sigma (d)^2}}{n}$, *d* being the deviation of any observation from the mean) the calculation of σ is very laborious. Now in the deviations of the means of the several divisions or classes which were used in constructing the diagrams, we have at hand substitutes for the deviations of the individual observations from the mean of the whole group; that is, we treat our divisional means as if they were actual individual observations. Where n is small (as in this case where it becomes uniformly 6) the formula for the standard deviation is usually written $\sigma = \frac{\sqrt{\Sigma (d)^2}}{n-1}$. With this correction made, the index of variability, *s*, obtained by this shorter method, becomes slightly larger than the S. D.[5] Probably the index of variability as calculated by this *s*-method is slightly less reliable than the orthodox S. D. It is sufficiently reliable, however, to serve its purpose in this paper—viz., to afford some sort of indication of the variability of the test and hence of the significance of the mean as a measure of general tendency.[6]

The P. E. of the Mean is $\frac{0.6745\sigma}{\sqrt{n}}$. In all cases, however, we have substituted *s* for σ. The P. E. of the Mean is the measure of the reliability of M as a measure of general tendency. The chance that the "true" mean differs from the calculated mean by three times the amount of the P. E. is one in twenty-three. To distinguish this P. E. from others, it is called in the tables that follow in this paper the E of M.

An amalgamation of two or more trials is generally more reliable than any single trial. Accordingly in most comparisons the amalgamation of the two testings is used. In not a few

[5] *Cf.* "Letters" test. First testing: $\sigma = 31.4$; by the shorter method, $s = 33.36$. Repetition: $\sigma = 34.56$; by the shorter method $s = 34.87$.

[6] This method of obtaining a measure of variability is not put forward as a general substitute for the S. D. Only when the divisional means are required (as here, for the diagrams) is any very material saving in computation effected. Where sextiles, octiles, etc. are indicated, one may use these to obtain a still easier and still more rough-and-ready measure of variability.

cases, however, the first testing is more significant and it seems best, therefore, to give the results for both testings as well as for the amalgamation. Correlations between tests and between tests and estimations of intelligence are, unless otherwise stated, based on the amalgamation of the two trials. There are two ways of arranging subjects in order of merit upon the basis of two trials: either the times (or other measure of ability) for the two trials, or the place-positions, may be added. The difference in result is small, slightly greater emphasis being laid by the latter method upon the differences between the two trials. As we deemed these differences to have their value, this latter was the method adopted. Correlations are thereby somewhat lessened.

In calculating averages, correlations, etc., the operation was carried to the point where the third decimal place would not be influenced by a change. The third place was dropped (according to the usual rule) only after all operations were concluded. Apparent discrepancies may thus occur. A correlation of .706 may have a probable error of .060; while one of .714 will have a probable error of .054. In the table both appear as .71, one with a probable error of .06, the other with a probable error of .05. The writer was careful to avoid mere arithmetical errors. In using the slide rule, certain inaccuracies may have crept in but he is quite confident that none of these affect the second decimal place.

V. Method of Provisionally Estimating Intelligence[7]

At the Group B school, the Headmaster drew up a list of the boys according to his estimate of their intelligence. His long experience as a teacher, his intimate acquaintance with all the boys and his clear understanding of the problem give one great confidence in the estimate. It must be kept in mind, however, that it is only an *estimate*. The low position given by him to one boy who did very well in the tests reduced very much many correlations with "intelligence." This particular boy impressed

[7] In correlating the results of the several tests with "intelligence," the term "Imputed Intelligence" is used both in the text and in the Table of Correlations.

the writer as being lazy—a quality which does not recommend itself to the pedagogue. The unusual nature of these tests stirred him into activity and showed his capabilities.

It was unfortunately impossible to secure a similar list for Group A. Recourse was had, therefore, to the position of the subjects on the school roll. As the boys were drawn from five forms and as no effort was made to dovetail them together, such a list is evidently an exceedingly poor method of estimating general intelligence. It may be used to confirm the results obtained with the more trustworthy Group B estimate or to lend some little weight to *a priori* expectations. Wherever a test shows a strong correlation with the school roll, we may assume some sort of correlation with intelligence.

VI. Apparatus, Procedure and Results of the Several Tests

1. "Plunger" test

The test was designed to measure quickness and accuracy of movement. A brass plate was fixed firmly upon a solid wooden block. Into this plate were set 30 bronze tubes 1.75 cm. high, 1.09 cm. in diameter. The tops were slightly beveled. A steel plunger set in a handle fitted nicely and easily into any given tube. The subject was instructed to push the plunger to the bottom of each tube in turn as rapidly as possible, holding the plunger more or less like a pen. Three complete rounds were made and time taken. Since the individual differences were very slight, the test was retained merely as a preliminary test in order to absorb the inevitable initial nervousness of some boys.

2, 3, and 4. "Alphabet" tests

(2="Letters" test; 3="Figures" test; 4="Alternating" test.)

In its present form the test is entirely new although it is a development of the alphabet test used by Burt (2). Enlarged replicas of the three parts of Fig. 1, each 9.5 cm. square and each on a separate sheet of paper were provided for every subject. The principle according to which these test sheets were constructed can be better explained after the procedure has been described.

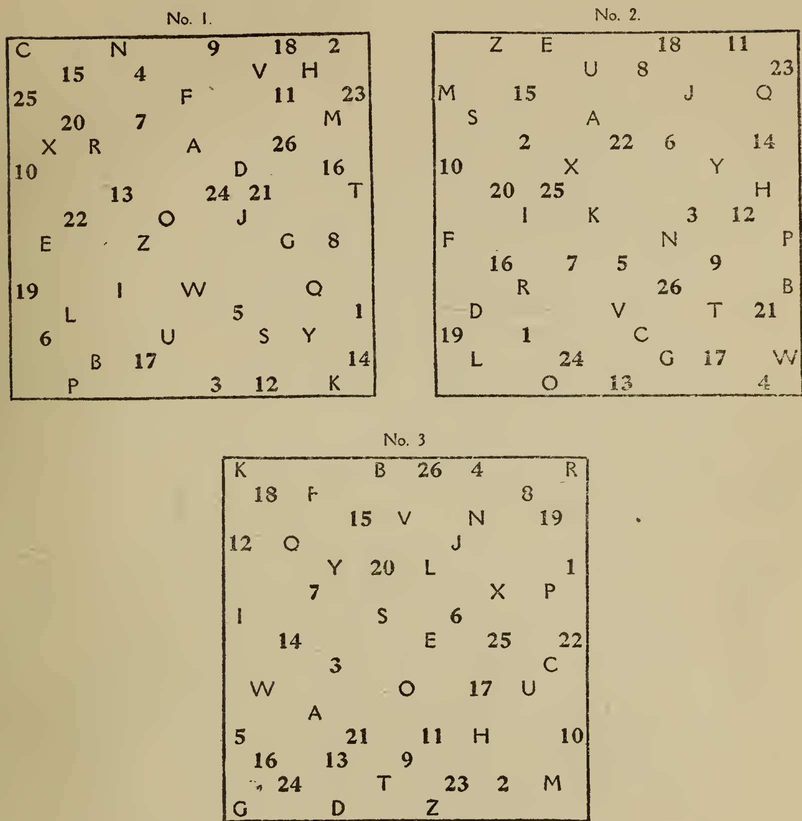


FIG. 1.

The subject was shown sheet No. 3 and the following instructions were given. "I am going to give you a sheet [No. 1] very much like this and I want you to cross off with this pencil the letters of the alphabet in order. This way [illustrating A to D] and so on to Z. Pay no attention to the figures. Call the letters aloud as you cross them off. And do it just as fast as you can. Is that clear? Then ready! Go!" A paper was always kept over the sheet to be used until the word "Go!" The time was recorded with a stop-watch in fifths of a second.

"On the next sheet [No. 2], you are to pay no attention to the *letters* but are to cross off the *figures* from 1 to 26 in *order*.

Notice the difference between 'I' and '1.' [Pointing.] Now see if you can't do the numbers faster than the letters. Ready? Go!" Time recorded as before.

"Now on the next paper [No. 3], I want you to cross off first a letter, then a figure. This way [illustrating on the sheet just finished, from A-1 to D-4] and so on to Z-26. Not two letters and then two numbers but first a letter and then a number. 'A' first, remember! You understand? Then as fast as you can go! Ready? Begin!" By means of a splithand stop-watch time was taken at the halfway point and again at the end. When the test was repeated (after about two months), the order of the papers was altered and the instructions were very much shortened.

The test sheets were constructed as follows. The square was divided into 25 square sections. "A" was located at random. In locating the others, no character was put nearer than two squares in any direction to the letter or figure just preceding or just following it, whether in the order of letters or of figures alone or of the two alternating. An effort was made to avoid putting successive characters on the same horizontal lines, especially from left to right, but this was not always possible, the preceding convention limiting very severely the number of squares in which the characters could be placed. Constructed in this way, sequences of any sort are avoided and the three pages are of almost exactly the same difficulty and may be upon occasion interchanged—as, for example, when false starts are made.

As for the results, the coefficients of reliability in Group A are .309 ± .105, .261 ± .108, .698 ± .06, for "Letters," "Figures" and "Alternating Letters and Figures" respectively.[8] In Group B the same coefficients were .565 ± .088, .71 ± .064, .778 ± .052. The lowest reliability coefficient is thus considerably more than twice its P. E.

The correlations of the various parts of this test with other tests may be seen by reference to the table of correlations (Table

[8] It is convenient to indicate the probable error of a correlation index by writing the index plus or minus the probable error. A good correlation should be at least twice, and in any doubtful case, at least three times its probable error.

XVI). The only noteworthy point is the fairly high correlation of "Letters" and "Alternating" with the "Analogies" test while the "Figures," despite its high correlation with "Letters" and "Alternating," shows no correlation with "Analogies" at all. That this is the case in both groups would indicate that it is hardly an accident. Moreover the probable error of the differences between the correlation "Figures-Analogies" on the one hand and "Letters-Analogies" on the other was determined. The formula for this is $P. E._{a-b} = \sqrt{P. E_a^2 + P. E_b^2}$. The differences and their errors in the above cases are: Group B, .316 ± .117 and .306 ± .17; Group A, .187 ± .16 and .279 ± .155. As the differences are thus materially larger than their probable errors, they can scarce be accidental. Just what quality the "Figures" test lacks and the other two possess, which is the ground of the correlation with the "Analogies," it is not easy to suggest.

In Group A only the "Figures" show a tendency to correlate with age but in Group B the tendency is quite pronounced in all three divisions of the test. This is probably attributable to the fact that the younger subjects in this group had not learned to repeat the alphabet or to count as well as its older members. At the Group A school even the youngest members seem to have been drilled in these two processes as well as, or even better than, the older subjects.

All three divisions of this test show substantial correlations with "Imputed Intelligence" in both groups.

Turning now to the performances, we find the superiority of Group A quite marked at every point (Curves, Figs. II, III, IV). The superiority is particularly marked among the poorer subjects, the best subjects of Group B not falling quite so noticeably behind those of Group A. Tables II, III, IV give the more significant figures for each group.

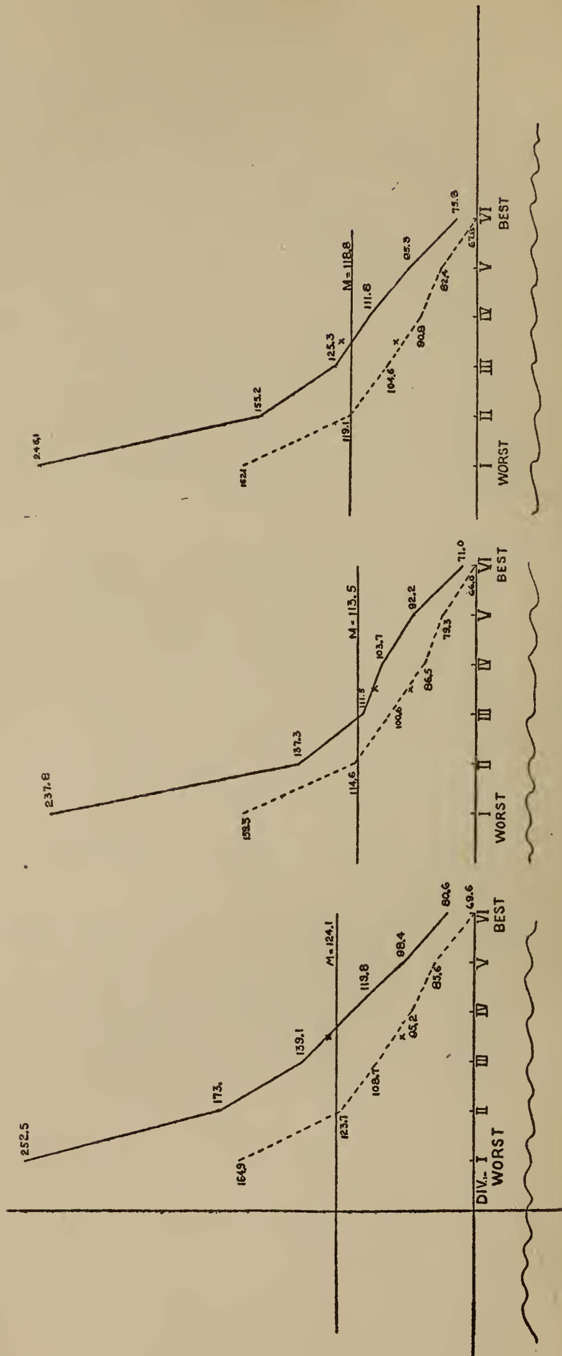


Fig. II.

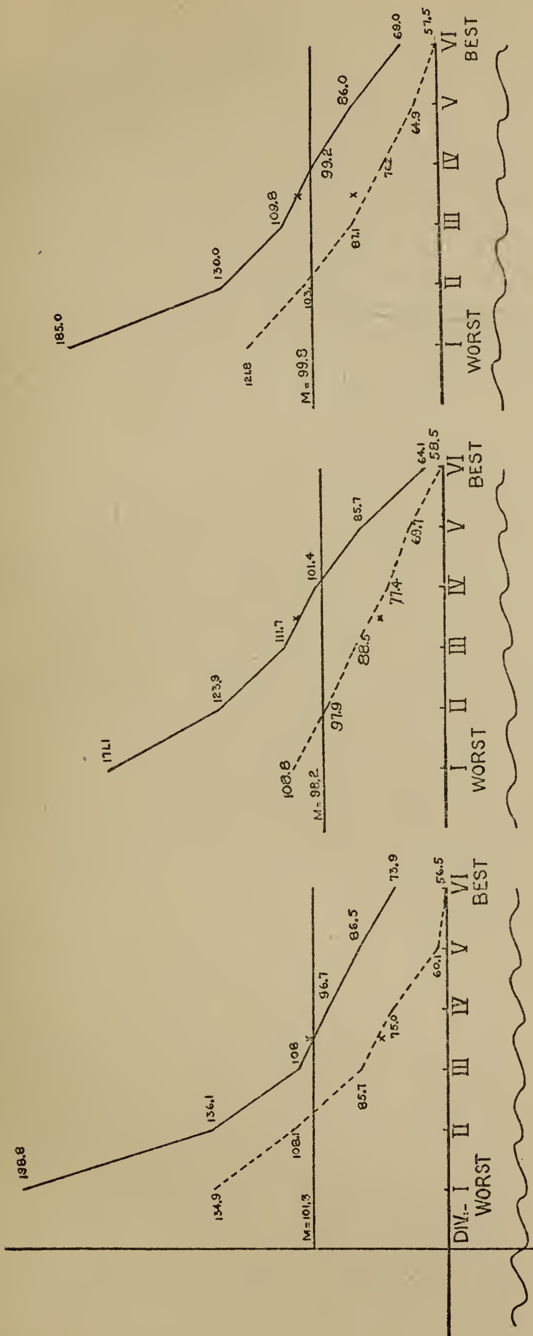

185.0
130.0
109.8
99.2
86.0
69.0
121.8
103.
87.1
76.2
64.9
57.5
M = 99.3
171.1
123.9
111.7
101.4
85.7
64.1
108.8
97.9
88.5
77.4
69.1
58.5
M = 98.2
198.8
136.1
108
96.7
86.5
73.9
134.9
108.1
85.7
75.0
60.1
56.5
M = 101.3
DIV.:- I
II
III
IV
V
VI
WORST
BEST

FIG. III

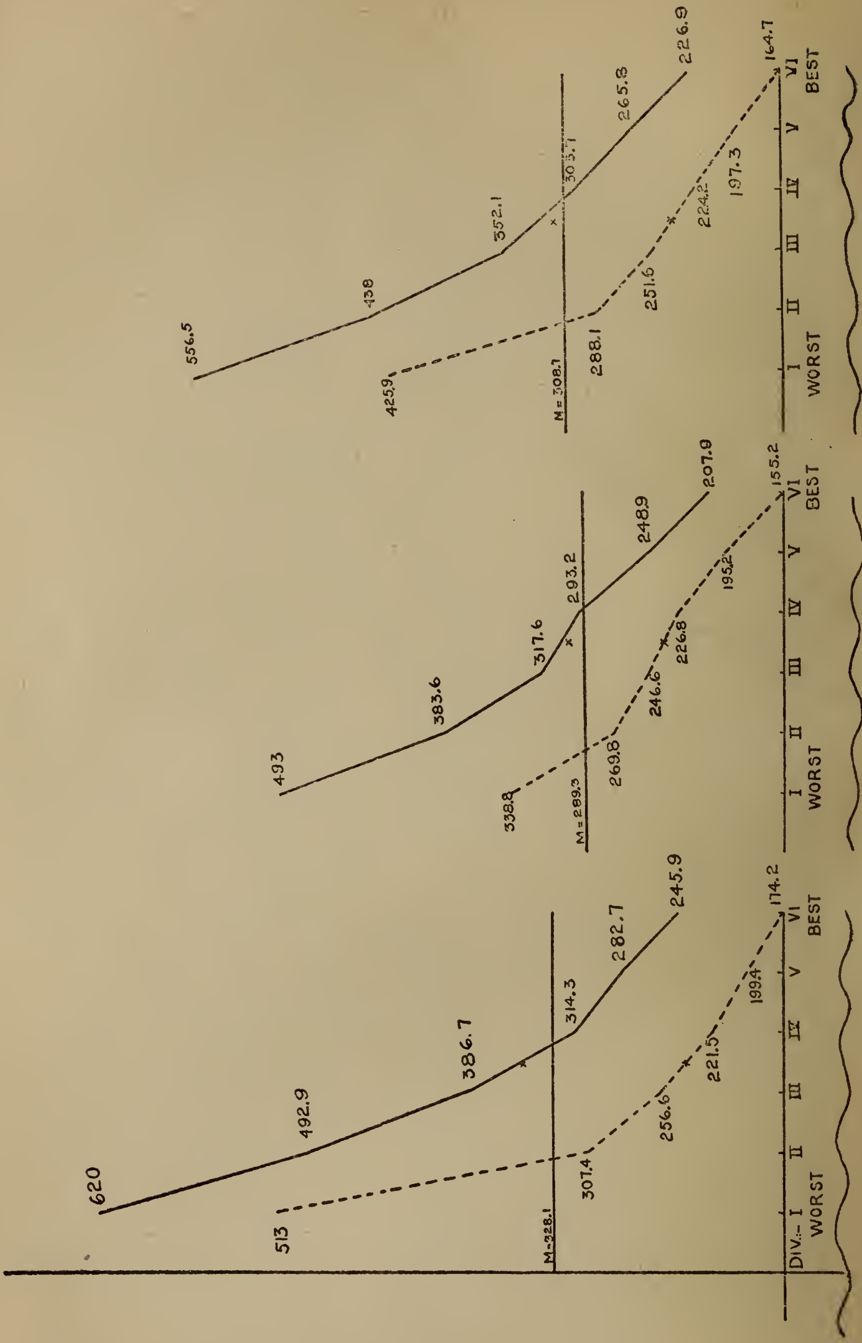

620
492.9
386.7
314.3
282.7
245.9
513
307.4
256.6
221.5
199.4
174.2
M=328.1
DIV.:- I
II
III
IV
V
VI
WORST
BEST
493
383.6
317.6
293.2
248.9
207.9
338.8
269.8
246.6
226.8
195.2
155.2
M=289.3
556.5
438
352.1
303.7
265.8
226.9
425.9
288.1
251.6
224.2
197.3
164.7
M=308.1

FIG. IV.

TABLE II

"Letters." First Testing. Time in seconds.[8a]

	Averages by Divisions[9] I	II	III	IV	V	VI	Best	Worst	Median	Mean	s	E of M
Group A	164.9	123.7	108.7	95.2	85.6	69.6	52.2	180.8	99.0	107.66	33.36	±3.70
Group B	252.2	173.	139.1	119.8	98.4	80.6	74.8	330.4	128.6	143.7	60.85	±7.37
Second Testing												
Group A	159.3	114.6	100.6	86.5	79.3	66.0	61.4	236.2	92.4	95.08	34.87	±3.87
Group B	237.8	137.3	111.5	103.7	92.2	71.0	63.	386.8	107.	127.19	59.15	±7.16
Amalgamation[10]												
Group A	162.1	119.1	104.6	90.8	82.4	67.8	59.9	173.6	101.	101.37	33.48	±3.71
Group B	245.1	155.2	125.3	111.8	95.3	75.8	69.3	357.6	124.2	135.46	60.41	±7.32

TABLE III

"Figures."[12] First Testing

	Averages by Divisions I	II	III	IV	V	VI	Best	Worst	Median	Mean	s	E of M
Group A	134.9	108.1	85.7	75.	60.1	56.5	46.	165.	78.8	88.36	30.19	±3.35
Group B	198.8	136.1	108.	96.7	86.5	73.9	62.8	249.	103.	116.87	45.47	±5.53
Second Testing												
Group A	108.8	97.9	88.5	77.4	69.7	58.5	44.4	118.4	79.8	83.38	18.57	±2.60
Group B	171.1	123.9	111.7	101.4	85.6	64.1	48.4	207.4	107.6	108.81	38.71	±4.69
Amalgamation												
Group A	121.8	103.	87.1	76.2	64.9	57.5	51.3	118.7	86.	85.87	24.18	±2.68
Group B	185.	130.	109.8	99.2	86.	69.	62.1	202.8	104.6	112.84	41.33	±5.02

TABLE IV

"Alternating."[12] First Testing

	Averages by Divisions I	II	III	IV	V	VI	Best	Worst	Median	Mean	s	E of M
Group A	513.1	307.4	256.6	221.5	199.4	174.2	161.8	690.	239.4	277.29	106.05	±11.76
Group B	620.	492.4	386.7	314.3	282.7	246.	214.8	737.	349.	388.67	101.20	±12.08
Second Testing												
Group A	338.8	269.8	246.6	226.8	195.2	155.2	138.2	391.8	237.6	238.66	64.89	± 7.2
Group B	493.	383.6	317.6	293.2	248.9	207.9	173.8	537.5	300.	328.66	107.16	±13.0
Amalgamation												
Group A	425.9	288.1	251.6	224.2	197.3	164.7	152.2	540.9	240.73	257.96	92.30	±10.24
Group B	556.5	438.	352.1	303.7	265.8	226.9	206.7	602.4	317.3	358.66	122.09	±14.78

[8a] For each mistake, 2 seconds were added to the actual time as penalty. This is certainly sufficient, since the subject was compelled to correct the error at the time as well. Very few mistakes were made.

[9] See page 273 for explanation of the method by which these divisions were made.

[10] Amalgamation. Under this head is given the average of the *subject* who did "Best," "Worst," or "Median," not the average of the two "Best," "Worst" and "Median" *results*. In case of ties (such as would result when subject A is first in one trial, fourth in the other, while B is second in one and third in the other), the mean of the two subjects is given.

[11] See notes to Table II, all of which apply equally to the "Figures."

[12] See notes to Table II, all of which apply here with one exception—

The clear-cut superiority of the Group A subjects may be explained on several grounds. Aside from the specific instructions, subjects were allowed to "gae their ane gait" unless, indeed, it was seen that a subject was fatally handicapping himself by a wrong procedure, and Group A boys showed not only in this, but in practically all tests, a flexibility in adapting themselves to the unusual conditions of the work which was frequently entirely lacking in the other group.[13] This showed itself primarily in the adoption of better methods; in this test, the combination in proper proportions of preliminary cursory with thorough search. We shall return to this question of adaptability later.

Zeal also played a considerable part in this test. As before pointed out, the attitude in Group B (with certain exceptions) was good-natured acquiescence in the instructions, while that in Group A was one of intense rivalry and desire to make a good record. In no other test was this so definitely manifested, and for the following reason. It was judged best for the sake of uniformity to require all subjects to say the name of the letter (or figure) as they crossed it out. Now while their whole attention was directed upon the search for a letter, the tone of their voice served as a complete betrayal of their frame of mind. One might note the attitudes of dwindling interest, spurts, annoyance, fatigue, determination, anxiety, etc. Particularly interesting was the attitude induced by prolonged lack of success, the subjects here falling into three classes: those who become hopeless and apathetic, those who take a fresh determination, and those who are uninfluenced. The best subjects (speaking broadly) of both groups belong to the second class, the majority of Group B to the last class, and a large proportion if not a majority of Group A to the first class. Their aim being to make a record or to beat a fellow, they became apathetic when this was felt to be impossible. The good natured interest of the Group B boys, while

mistakes were rather frequent. Group A averaged .5 and .36 mistakes per subject for first and second trials respectively. Group B averaged .61 and .35 mistakes per subject for first and second trials respectively.

[13] It is possible that the difference previously remarked between "Letters" and "Alternating" on the one hand, and "Figures" on the other, may be due to the greater novelty of the two former.

not so great, was also not so easily cast down. Nevertheless, although zeal may sometimes overreach itself, the group possessing it has a distinct advantage.

As a result of our experience, we should suggest one or two improvements in the "Alphabet" tests. "Figures" should be given before "Letters." In "Alternating Letters and Figures" the test should be stopped midway—after "13" has been crossed out. This for several reasons: the time and patience of the experimenter would be saved without serious loss, if not a positive gain, in efficiency; most of the mistakes arise from subjects not knowing the order of the latter part of the alphabet; and for young subjects, the strain on the attention in a test such as this, lasting up to ten minutes or more, is too great.[14] Quite possibly it would be well to adopt the same procedure for the other two parts of the test.

It is believed that our form of "Alphabet" tests is an improvement over the "Alphabet Sorting" test used by Burt. Chance arrangements are avoided and the motor element is reduced to a minimum. Printed sheets also save the experimenter's time. As it stands, the test doubtless measures primarily quickness of observation or perceptual discrimination. Attention, concentration, zeal are large factors, and so it would seem, is adaptability, though this is not so obvious nor so demonstrable. A further element of the greatest importance is introduced in the "Alternating" test—the power of dividing the attention, or perhaps more accurately, of rapidly alternating the attention, between two tasks. In all but one case (out of over 150 tested by the writer) the time taken for the "Alternating" greatly exceeds the sum for the "Letters" and the "Figures" separately. It was found that the loss introduced by carrying on the two tasks at

[14] For those who would like to compare results under such procedure with ours we append the following table:

TABLE V

"Alternating Letters and Figures" to M — 13 only

Time in Seconds

	First Testing				Second				Amalgamation			
	Best	Worst	Aver.	Median	Best	Worst	Aver.	Median	Best	Worst	Aver.	Median
Group A	85.8	346.6	146.39	126.2	81.8	235.0	136.7	130.4	84.6	283.3	141.54	128.0
Group B	116.6	372.2	201.46	188.0	75.0	277.4	168.36	173.4	106.9	294.8	184.91	174.0

once amounted to almost one-third, being about 3% less in Group A. This difference, between the groups, in view of the crudity of the method of computation (viz., subtraction), is hardly significant.

We may sum up as follows: The alphabet tests are practicable, reasonably reliable, and yield fairly high correlations with "imputed intelligence." Group A is superior at every point and in every respect, in most respects very markedly so.

5. "Tapping" test

Tapping has been used as a test by several experimenters but the apparatus (designed by Dr. Schuster) used in this test is partly new. Four Veeder stroke-counters closely resembling an ordinary cyclometer were fastened securely to a block of wood clamped to a table. The tapper was of wood about the size of a fountain pen barrel, provided with a substantial rubber tip to prevent slipping from the stroke-counter, and with a bulb-like enlargement near the lower end to provide a secure grip.

The subject was seated a little to the right so that his arm would not be impeded at any time by his body. After showing the subject how to hold the tapper like a pen but rather more vertically, a practice spell of ten seconds was given. All the subjects used a full forearm movement, nearly all of them naturally; the others were asked to use this method in order to keep the conditions uniform. While the subjects rested, after the practice, the instructions were completed. He was to tap as fast as ever he could on each register until told to change to the next, which he was to do without stopping. Emphasis was laid on top speed from the very beginning and the necessity of keeping it up right to the end. Fifteen seconds were given on each register, and the number tapped on each register was separately recorded. A post of the same height as the counters, and at the same distance from the first as the distance between counters, was provided from which to start in order to equalize time conditions on the first counter with those on the others.

The tapping was given in four periods in the hope of obtaining a figure measuring the subjects' "persistence." The effort was very great and not a little painful. In spite of this, however,

most subjects were much interested in the test; and physical fatigue, sheer inability to proceed, can hardly have played any predominating part in so short a series. One must confess to great disappointment with the results obtained. Almost every possible way of treating the figures was used in order to get significant measurements. The falling off from first to last may be a significant figure in itself but it is not a fair measurement of the subject's ability to resist the tendency to slow up. For clearly, almost everything here depends on the initial rate; one subject starts with a great rush which it would be unjust to expect him to continue, another disregards the instructions and starts at a more leisurely rate. If we could assume that each subject started at his maximum rate, then and then only would the falling-off be a true measure of persistency. But such an assumption is unjustified. Regretfully, therefore, we must give up our hope of measuring this most valuable quality. Persistency or rather the ability to maintain the pace, on the one hand, and initial rate, on the other, tend to be negative functions of each other, and in this test they cannot be separately measured.

There remains the total number of taps made in one minute as a measure of the speed of movement plus persistency—a sort of total efficiency. The reliability is very high (.83 ± .04 for Group B and .66 ± .07 for Group A). On the other hand, the correlations with other tests are either negative or practically negligible (see Table XVI). In Group B only two are large enough to be accounted of any significance—a small positive correlation with "Dotting" and a slightly larger negative correlation with "Narrative Memory." In Group A, we have a run of negative correlations, some of them considerable. Again, "Dotting" alone correlates with "Tapping," this time fairly well. This tendency for these two tests towards concomitant variation is interesting in view of the fact that both show either negative or no measureable correlation with intelligence. In Group B, tapping shows an index of correlation with "Imputed Intelligence" of only .026—very little more than the P. E.; in Group A, the correlation is — .388 ± .096.

These correlations are noteworthy in view of the fact that

this is the one test in which the Group B boys, from the laboring class, are unequivocally superior to those of the other group. The following tables and the accompanying diagrams (Fig. V) show the extent of this superiority.

TABLE VI

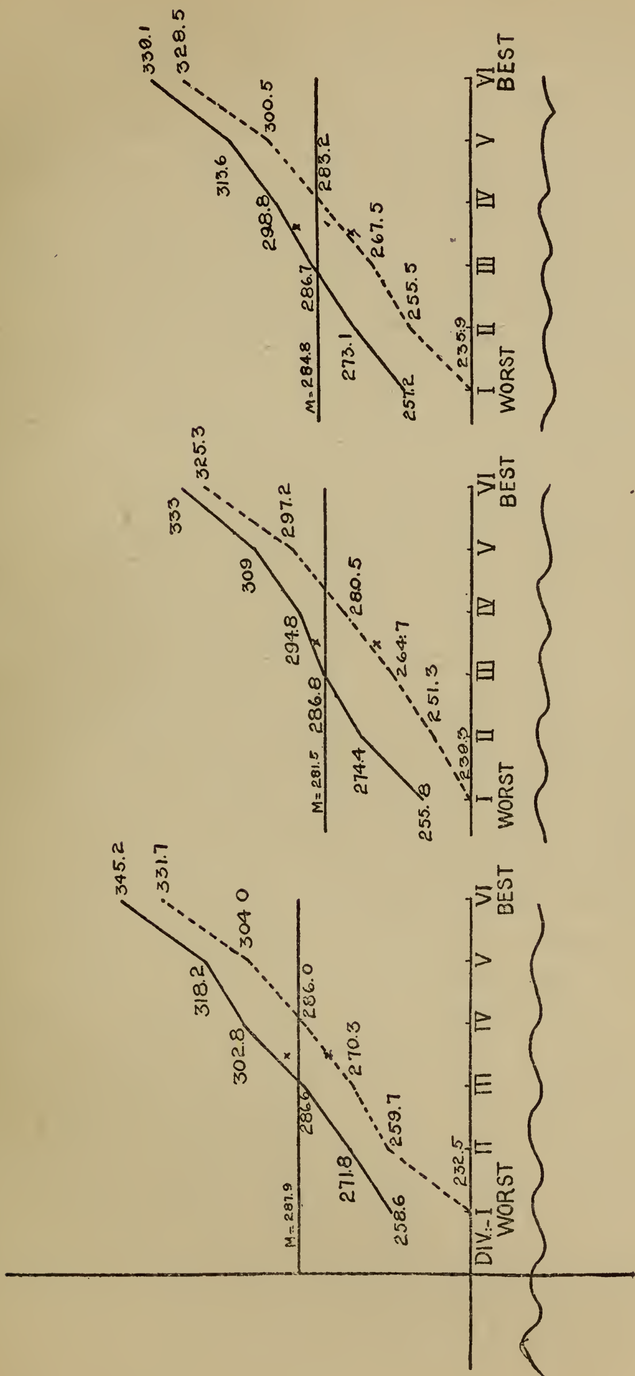
"Tapping." Total No. tapped in one minute

	Averages by Divisions I	II	III	IV	V	VI	Best	Worst	Median	Mean	s	E of M
First Testing												
Group A	232.5	259.7	270.3	286.	304.	331.7	346.	202.	278.	280.	34.8	±3.8
Group B	258.6	271.8	286.6	302.8	318.2	345.2	364.	251.	292.	297.	31.7	±3.8
Second Testing												
Group A	239.3	251.3	264.7	280.5	297.2	325.3	343.	158.	270.	273.	31.7	±3.5
Group B	255.8	274.4	286.8	294.8	309.	333.	348.	245.	290.	292.	26.6	±3.2
Amalgamation												
Group A	235.9	255.5	267.5	283.2	300.5	328.5	345.	180.	274.	276.5	33.2	±3.10
Group B	257.2	273.1	286.7	298.8	313.6	339.1	356.	255.	291.	294.5	29.2	±3.5

The superiority of Group B is rather surprising. Although they are a healthy and wholesome lot of lads, they are not so lively and full of spirits as those of the other group. Both schools give regular exercise as well as competitive games but these are more extended at the Group A school. In spite of this we have a very marked and clear cut superiority in a test whose coefficient of reliability is high.

6. "Analogies" test

On the whole this is probably the most satisfactory test used in this investigation. The test was suggested to the writer by Mr. Cyril Burt, who also supplied many of the analogies used. It has since been described by Wyatt (10) and Yerkes (11). In this test the subject was given three words and required to supply a fourth word. Between the first two words certain logical relations obtained; the required fourth word must bear the same logical relation as the third. Obviously here we have reasoning of a fairly high degree, both analytic and constructive. The relations were, however, generally very easy to comprehend and the subject matter was familiar. The test was given as a group test but every possible effort was made to see that each subject understood what he was to try to do before beginning.



339.1
328.5
300.5
313.6
298.8
283.2
267.5
286.7
255.5
M=284.8
273.1
235.9
257.2
325.3
333
297.2
309
280.5
294.8
264.7
286.8
251.3
M=281.5
274.4
239.5
255.8
345.2
331.7
304.0
318.2
286.0
302.8
270.3
286.6
259.7
271.8
232.5
M=281.9
258.6
DIV.:-I
I
II
III
IV
V
VI
WORST
BEST

Fig. V.

The following was the explanation which was, however, supplemented by questions and answers both on the part of experimenter and subjects.

"Most of you have done proportion in arithmetic, I suppose. We are going to have a new sort of proportion. Instead of numbers we shall have words. For example, as a father is to his son so is a mother to her—what? [All examples were written on the blackboard so as to familiarize the subjects with the form. The correct answers were also written in.] 'Daughter.' Right. As a tree is to a sapling, so is a man to a—'boy.' Let us take another. Book : page :: congregation : ——. Hands. 'People?' No, a person. You see it was only one page so it will be only one person. Now let's see what we have been doing. We notice first that the first two words go naturally together; they are relations or are somehow connected, aren't they? In the first one it was a case of parent and child, in the next of old and young, then of whole and parts. Now in the list of words you are to work with, the first two will always be connected something like this. The things the words name will always have something to do with each other, just as the things above named did. Then there will be the third word and a blank space. What you are to do is to fill in the blank space. Now just any word won't do. It must be a word that is like or different from the third word, connected with the third, just exactly the same as the second is like, different from or connected with the first. Let us take one or two more examples. Suppose you had: North : South :: East : ——— 'West.' That's right. That's easy, isn't it? Now notice this one pretty carefully. North : Pole :: Left : ———. 'Right?' No. That is wrong. I put this one up to fool you. It is an impossible question. There is no answer. I just wanted you to notice that you can't answer these unless you think. We get so used to 'left-right, left-right' that we answer 'right' without thinking; that won't do. You must *think* on *every one* of these. But notice this. On the papers you have there are no 'foolers.' *There is an answer to every single question.* Only to get the answer you must *think.* Ask yourself how the first and second are connected and try to find a word as like the third as the second is like the first. Now if any one

is not entirely clear what he is to do, I hope he will ask me a question now. This is hard and you want to get it right, so now is the time to ask. When I give you the command 'Turn' all should turn their papers over and begin work. Don't take too long over any one set but go on to the others and come back to it. Finish as many as possible but it is more important to get them right. Ready? Turn!"

Shortly after they had begun, the experimenter announced that he was ready to explain the meaning of any words. At five minutes they were warned to be at least half through. Eight minutes were allowed with slight extensions for the younger boys, who wrote slowly. The writer acted as experimenter throughout this test. The instructions for the second trial were as follows:

"We are going to have another set of those papers in which you fill in the missing fourth word. And I want to call your attention to some common mistakes. No. 3 on the old paper is a good example. 'Weak : Strong :: Sickness : ———.' Many of wrote 'Healthy.' That won't do. Healthiness or health is what is related to sickness as weak is to strong. Or if we had had 'Ill' then 'Healthy' would have been right. Good : Better :: Bad : ———. Would you believe me when I say many of you wrote 'Worst'? Of course it is 'Worse.' Bricks : Walls :: Tiles : ——— 'Roofs.' So many had just 'Roof.' But 'Walls' means more than one wall and 'Roofs' must correspond.

"Now let's have one more illustration of the way to go about the test. Suppose we have England : London. What is the relation? No, that is too easy. Say Sheffield : Steel. What is here the relation? the connection? City to its chief product. Then if we put down Nottingham here in the third place, all we have to do is to put in Nottingham's chief product which is lace. You must *first* make out the connection of the first pair and the rest is generally easy. Has any one a question? Well, you may not have as long a time as before, so look sharp. Ready? Turn!"

Boys whose first trial had been conspicuously bad were questioned until they seemed to get the idea. It is not an easy matter to explain this test so that the children will understand the nature

of the task. Those materially younger than the boys who did these tests for us conspicuously fail.

One was scored against each subject for each mistake. Mistakes merely in the grammatical number of the reply counted only ¼. The following two lists of analogies were used for the two trials.

First List

1. Eating : Hungry :: Drinking : ————
2. Stationer's : Notepaper :: Butcher's : ————
3. Weak : Strong :: Sickness : ————
4. Black-Lead : Pencil :: Ink : ————
5. Good : Better :: Bad : ————
6. Shirts : Flannel :: Boots : ————
7. Head : Headache :: Tooth : ————
8. Master : Servant :: Teacher : ————
9. Bricks : Walls :: Tiles : ————
10. Preaching : Clergyman :: Healing : ————
11. Birds : Flying :: Fishes : ————
12. 5 : 10 :: 10 : ————
13. Seeds : Plants :: Eggs : ————
14. Needle : Prick :: Knife : ————
15. Laundress : Linen :: Bootblack : ————
16. Dinner : Mid-day :: Supper : ————
17. End : Death :: Beginning : ————
18. Forget-me-not : Blue :: Poppy : ————
19. Clothes : Tailor :: Boots : ————
20. Hills : Mountains :: Lakes : ————
21. Necklace : Neck :: Bracelets : ————
22. Eating : Table :: Writing : ————
23. Drinking : Cup :: Eating : ————
24. Horses : Carts :: Engines : ————

Second List

25. Sofas : Cushions :: Beds : ————
26. House : Door :: Body : ————
27. Miaow : Cat :: Bow-wow : ————
28. Ocean : Ship :: Air : ————
29. Hair : Man :: Fur: ————
30. Doctor : Illness : : Dentist : ————
31. Mother : Child :: Cat : ————
32. Wires : Telegrams :: Postmen : ————
33. Sponge : Slate :: India Rubber : ————
34. Half : Whole :: Semi-Circle : ————
35. North : South-West :: South : ————
36. Cricket : Bat :: Tennis : ————
37. Fold : Sheep :: Stables : ————
38. Coals : Mines :: Wood : ————
39. Getting Up : Sunrise :: Going to Bed : ————
40. A Carpenter : Wood :: A Smith : ————
41. Buttercups : Daisies :: Yellow : ————
42. Grass : Hay :: Wheat : ————
43. Drunkards : Drinking :: Gamblers : ————
44. Please : Thank You :: Asking : ————
45. Thread : String :: String : ————
46. Geography : Astronomy :: Earth : ————
47. University : School :: Students : ————
48. Twelve : One :: A Shilling : ————
49. Time : Clock :: Lengths : ————
50. Trout : Fish :: Sparrow : ————

The reliability of the test is unusually high. (Coefficients: Group B, .78 ± .05, Group A, .65 ± .071.) The various correlations may be seen by reference to Table XVI. The especially high correlations with "Imputed Intelligence" in both groups should be noted. In our judgment this is not relatively high enough. No single test is a fair measure of intelligence but we feel that this test comes nearer such than any other we are familiar with. Its chief defect is a certain unfairness to the better subjects who never take anything like the full time for the test.

The superiority of Group A is not quite so striking as in one or two other tests but is decided and unequivocal. One should notice the way in which the two curves approach each other at the better end. An unusual feature of the test is that the best subject of all was a member of Group B. Table VII gives all the more significant figures. The curves appear in Fig. VI.

TABLE VII

"Analogies." Results in No. of errors

	Averages by Divisions											
	I	II	III	IV	V	VI	Best	Worst	Median	Mean	s	E or M
First Testing												
Group A	11.04	2.83	1.25	1.	.42	.04	0	22.	1	2.82	4.17	±.462
Group B	18.4	9.	5.4	4.3	2.5	1.0	0	22.5	4.5	6.7	6.32	±.757
Second Testing												
Group A	9.79	6.5	4.25	2.75	1.71	1.0	1	12.5	3.	4.24	3.0	±.333
Group B	19.1	10.55	8.25	6.55	4.05	2.0	0	24.5	7	8.36	6.04	±.725
Amalgamation												
Group A	10.83	4.67	2.75	1.88	1.06	.52	.5	16.56	2.5	3.53	3.82	±.425
Group B	18.75	9.78	6.82	5.42	3.28	1.5	0	22.	6.25	7.53	6.17	±.74

It is clear from our experience that the test may be decidedly improved. The list of analogies as here given is far from perfect. To avoid ambiguity, purely verbal associatons, etc. we suggest the following substitution:

For no. 2 Note paper : Stationer's : : Meat : ————
" " 5 Better : Good : : Worse : ————
" " 20 Lakes : Seas : : Hills : ————
" " 22 Writing : Desk : : Eating : ————
" " 23 Cup : Drinking : : Plate : ————
" " 25 Cushions : Sofas : : Pillows : ————
" " 26 Body : Mouth : : House : ————
" " 36 Racquet : Tennis : : Bat : ————(Baseball or Cricket.)
" " 37 Sheep : Fold : : Cattle : ————
" " 41 Buttercups : Easter Lillies : : Yellow : ————
" " 49 Lengths : Ruler : : Time : ————

Nos. 12, 16, 39, and 46 should be entirely omitted. Nos. 22 and 23 should be separated. It will be noticed that most of these changes make the test easier; this is not, however, the aim but merely to insure fairness. Certain of the remaining questions are still difficult enough to make even adults think twice.

A more radical change in procedure would be to give the test to the subjects individually rather than in groups and to limit the time allowed for each reply—certainly not more than 10, and probably not more than 7 or 8 seconds. For such procedure the "analogies" could be printed on separate cards. The advantage of the change would be to give a fairer comparison—since the

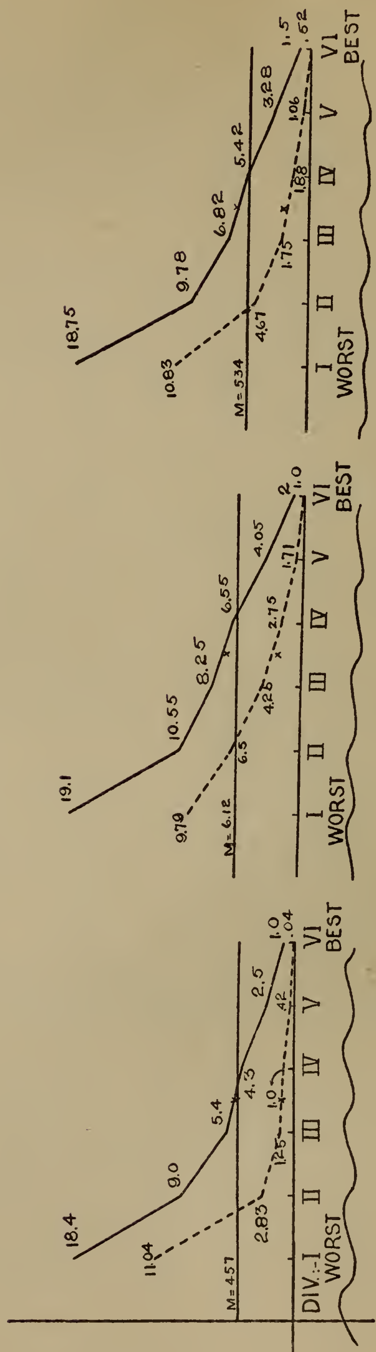

DIV.:-I
WORST
BEST
I
II
III
IV
V
VI
M=457
M=6.12
M=534
18.4
9.0
5.4
4.3
2.5
1.0
11.04
2.83
1.25
1.0
.42
.04
19.1
10.55
8.25
6.55
4.05
2
9.79
6.5
4.25
2.75
1.71
1.0
18.75
9.78
6.82
5.42
3.28
1.5
10.83
4.61
1.75
1.88
1.06
.52

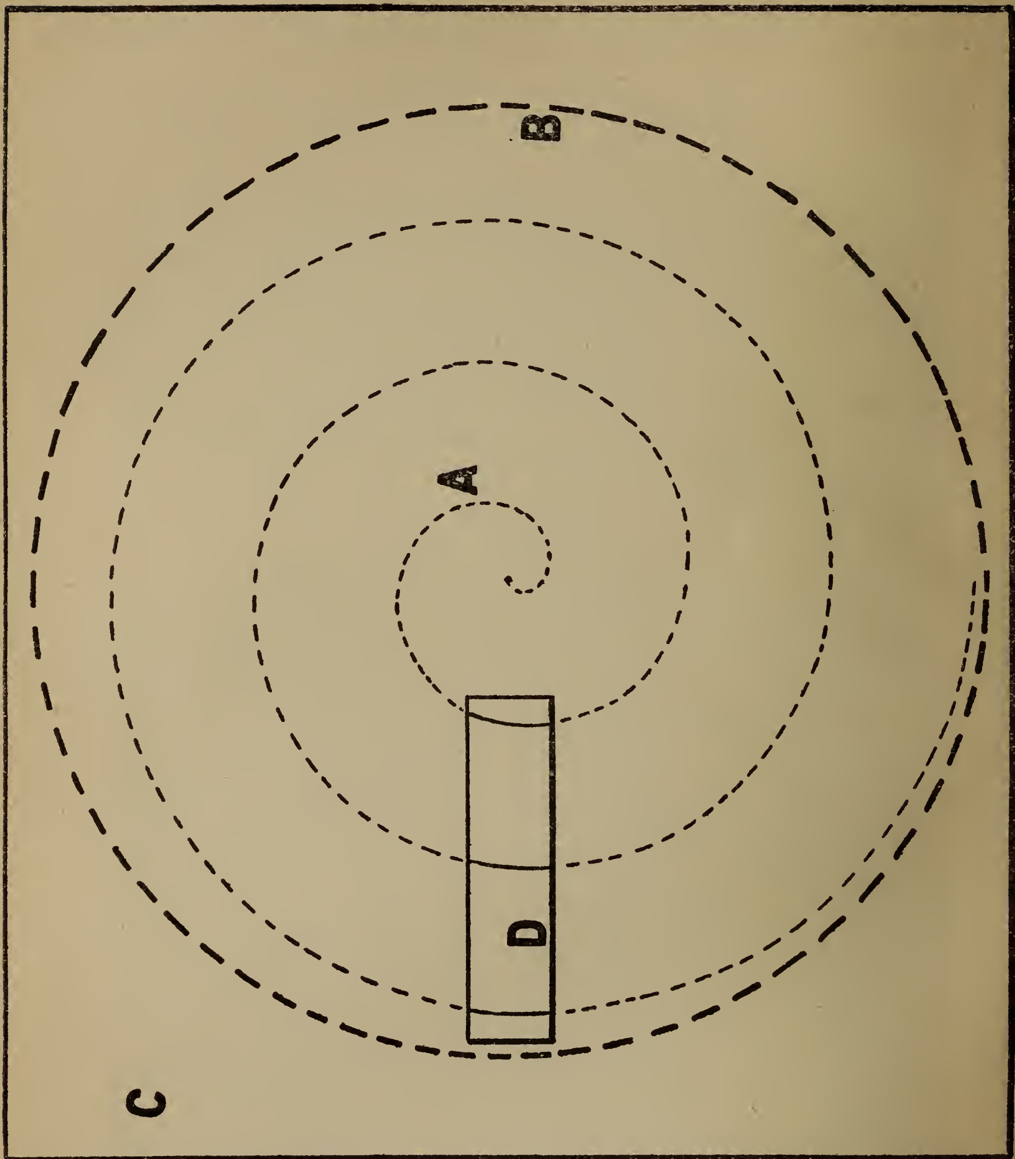
Fig. VI.

better subjects take much less time than the poorer. Our experience with the "Discs and Circles" test described below leads us to believe that a counter for recording the number of errors during the progress of the test would then be found almost necessary.

The explanatory instructions need careful revision. Ours were the result of our experience at the parish school but further experience proved them insufficient. It is immensely difficult to explain this test to children because of one's tendency to describe in abstract terms. Even those children, it is to be feared, who grasped the nature of the task from the examples, failed to understand parts of the instructions. It would be better to illustrate (almost entirely by numerous examples, carefully selected) the different cautions to be enforced.

7. "Dotting" test

This test was suggested by Professor McDougall. The apparatus was improved first by Dr. Rivers and then by Dr. Schuster. The form used by us may be called the Disc-Dotting machine (see Fig. VII). An irregular row of small circles 2 mm. in diameter is printed spirally in red on a paper disc 326 mm. in diameter. The spiral makes altogether two complete turns. The circles (which have their centers indicated by a red dot) are carefully arranged so that their succession shall be as irregular as possible. The interval between successive circles, in the direction of motion of the spiral, was 5 mm.; the lateral deviation was never more than 7 mm. The circles were thus clustered irregularly about the "true" line of the spiral (the latter represented by A in the figure). The paper disc is mounted on a horizontally disposed plate (B) made to rotate by clock-work at a constant rate. In our case a gramophone motor—very silent in movement—was used. A cover (C) is supported above the plate in which a slot (D) one inch wide is cut in such a position and of such a length that, as the disc rotates, every point in the spiral row of circles presents itself to view through the slot. The subject rests his wrist on the cover and aims at the circles as they appear through the slot at one edge and are carried out of sight at the other. He starts at the inner end of the spiral and makes



his way outwards over it as its whole length presents itself point by point through the slot. This compels him to aim at the circles at a gradually increasing rate. At the inner end of the spiral

an angular movement of $45°$ brings seven circles out from under the edge of the slot; at the outer end of the spiral 24 circles are shown by the same angular movement. Thus the subject has to aim more than three times as fast as at the inner end. The clock-work was set to make a complete rotation in 40.5 seconds. Thus at the beginning the subject has to aim 83 times per minute, at the end of the spiral, 286 times. The former is well within the powers of any normal subject over ten or eleven years of age, the latter is too difficult for all children and for practically all adults. Hence it must happen that somewhere between the beginning and the end a point is found for each subject at which the rate of aiming becomes too rapid for him. This point is the measure of the dotting ability of the subject.

The instructions for this test were brief and of less importance than for most tests. The subject here learned preeminently by doing and by seeing. Each saw the test performed by one of his fellows, and thus grasped the general idea (which was nevertheless enforced by precept) that the circles come faster and faster as one works one's way out. The boys were told to aim at the circles as soon after they came into sight as possible—i. e., to keep the work at the "upper" edge of the slot or "window." They were not to try to correct errors ("which only count as two errors instead of one and get you behind"), not to stop until given permission, and not to give it up as they might be doing better than they thought. A comfortable and convenient posture, grasp of the pen, etc., was prescribed. The subject was seated fairly high above the machine, the light in front and a little to the left. If a subject became "rattled" and was obviously losing out, he was told to skip a few and take a fresh start. If he was able to maintain himself, no account was taken of the temporary lapse. In order to give subjects the same amount of practice, they were all forced to finish the entire spiral the first time but on the second testing were stopped when their limit had obviously been reached.

The subjects were graded according to the breakdown-point. This was estimated as follows: A "miss" was a circle not hit and any mark on the paper not obviously directed at some circle.

All extra marks were thus "misses" but marks aimed at a circle and not hitting it counted only as one miss, not one for the circle and one for the mark. Beginning with the first "miss," ten circles forward were counted. A breakdown consisted of five misses out of ten. If, however, the subject could then recover and for two successive series of ten circles keep up with the machine, the breakdown was not counted. The eventual breakdown was scored from the beginning of that series of ten in which it occurred. The entire spiral was divided into 23 sectors by radial lines. After the breakdown, the subject was given a grade according to the nearest fourth of these sectors. The range was from 6½ to 20½. It will be noticed, however, that a difference of one-fourth of a sector amounts to a great deal more at 17 to 17¼ than at the less peripheral 8½ to 8¾. This does not affect the arrangement into ranks for the better subjects are more sharply differentiated anyway, but it does affect the comparison of the two groups. The number of circles was therefore counted for each quarter-sector and the score given was according to the number of circles aimed at by the subject before his breakdown. (Not, it is to be remarked, the actual number hit, the sporadic misses up to the breakdown-point being of no importance.) The range is thus from 57 to 292 circles aimed at.

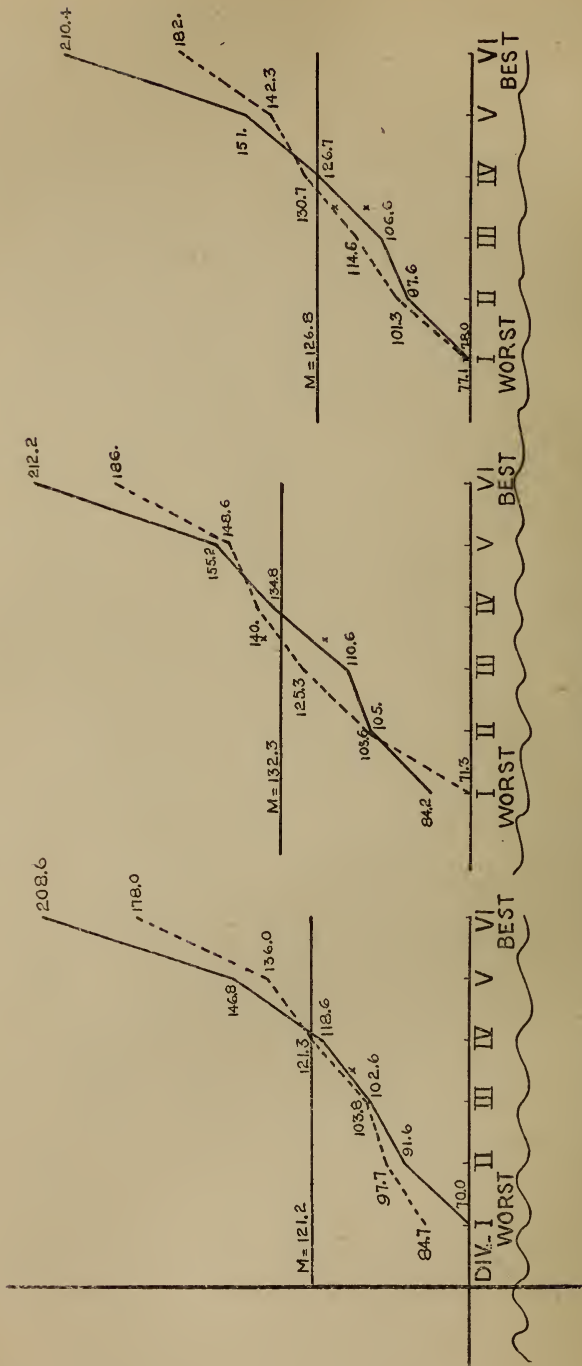
The reliability of the test is very high. In spite of its being given at rather widely different intervals and by different experimenters, the coefficient of reliability is $.729 \pm .061$ in Group B and $.723 \pm .054$ in Group A.

The table of correlations records some very interesting and baffling results. In Group A, "Dotting" shows a high positive correlation with "Tapping." Its correlations with other tests are identical or closely similar to the correlations between those same tests and "Tapping." This seems clearly to point to the conclusion that the same element largely determines excellence in these two tests—most probably the speed of movement factor. By the method of multiple correlation, however, we find that "Dotting" has a negative correlation of $-.20 \pm .11$ with "Immediate Memory," even when "Tapping" is constant. There is zero correlation with "Analogies," "Narrative Memory" and

"Imputed Intelligence" and positive correlation of .24 ± .11 with "Spot Pattern" and "Related Memory." We have here, then, some function not present in "Tapping" and one which seems to correlate more highly with the more "intellectual" tests.

When we turn to Group B, we find some curious changes. All of the correlations are positive, several being considerable. The correlation with "Tapping" is no longer marked. If by the method of multiple correlation, we render the element common to "Tapping" and "Dotting" constant, we scarcely affect the amount of the correlations of "Dotting" with other tests. In Group B, then, the speed of movement factor, which operated in "Tapping," does not seem to be of primary importance in the correlations of "Dotting." The determining factor is some function of but moderate yet appreciable correlation with intelligence.

That a test may exercise one function in one group, another function in another, is clear enough when the two groups are quite different in respect to training or native ability. Taking the lower half of Group A, based on "Imputed Intelligence," it was found that "Dotting" correlated with "Imputed Intelligence" with an index of .19 ± .16. It would seem, therefore, that as general intelligence decreases, "Dotting" becomes more and more correlated with it, i. e. its diagnostic value is greater for the less intelligent. The diagrams (Fig. VIII) give some hint of this, for this is the only test in which the lines in the diagrams cross. That is, on the first testing, the poorer subjects of Group A are better than the poorer subjects in the other group. As we approach the mean, they are more and more equal until the relative positions are changed and we find the two best divisions of Group B quite noticeably better than those of Group A. The mean for the latter group is slightly larger and a smaller percentage fall below the amalgamated mean for the two groups. This slight superiority is increased upon the second testing. Table VIII gives the more important figures.



DIV. I
WORST
II
III
IV
V
VI
BEST
M=121.2
M=132.3
M=126.8
208.6
178.0
136.0
146.8
118.6
121.3
102.6
103.8
91.6
97.7
70.0
84.7
212.2
186.
148.6
155.2
134.8
140.
110.6
125.3
105.
103.6
71.3
84.2
210.4
182.
142.3
151.
126.7
130.7
106.6
114.6
97.6
101.3
78.0
77.1

Fig. VIII.

TABLE VIII

"Dotting"—Results in No. of circles before breakdown

First Testing

	Averages by Divisions I	II	III	IV	V	VI	Best	Worst	Median	Mean	s	E of M
Group A	84.7	97.7	103.8	121.3	136.0	178.0	207	63	108	120	12.50	±1.39
Group B	70.0	91.6	102.6	118.6	146.8	208.6	292	57	108	122.5	20.14	±2.44
Second Testing												
Group A	71.3	105.0	125.3	140.0	148.6	186.0	221	66	138	131.7	14.6	±1.62
Group B	84.2	103.6	110.6	134.8	155.2	212.2	247	57	119	133	18.72	±2.26
Amalgamation												
Group A	78.0	101.3	114.6	130.7	142.3	182.0	205	69	121.5	125.8	13.4	±1.49
Group B	77.1	97.6	106.6	126.7	151.0	210.4	244	57	115	127.8	19.32	±2.34

It is obvious from the above that the two groups are practically even in their achievements in this test. The diagnostic value of the test for children of ordinary intelligence is, to say the least, doubtful. Certain of the very dull subjects in both groups were very bad at this test but with them were bracketed several of the brightest.

The test was used in the hope of measuring voluntary attention. One of the workers in the Oxford Psycho-Physical Laboratory has tested herself daily for long periods, using the McDougall-Rivers Dotting Machine, and has found it a most delicate and responsive test of attention. The writer is convinced, however, that for children it is not so effective and he questions its availability for general use as a diagnostic *test* even for adults. The subject above referred to had, by long practice, reached her maximal performance. Any lapses of attention resulted in a decrease from the normal. During the process of establishing such a normal performance, however (and it was of course in such a formative period that our records were taken), other than attention factors, chiefly perhaps manual dexterity and coolness, play a greater part in the determination of rank.

8. "Spot Pattern" test

The "Spot-Pattern" test was first described by Burt (2) and was devised by Professor McDougall. The portable tachisto-

scope also devised by him is described by Burt in the following terms: "It consists of a vertical stand of wood attached to a horizontal base by a hinge—which allows it to fold forward when not in use—and by a detachable spring, which keeps it upright when hooked to it at the back. In the upright stand is cut a circular aperture about 7 cm. in diameter; and at the back of this is screwed a Packard-Ideal Shutter with an aperture of 2¾ inches in diameter; while the front—for ordinary tachistoscope experiments—is covered with a semi-transparent card, sliding in grooves fixed on the face of the stand, and bearing printed in its hinder surface the object to be shown by means of the tachistoscope. The card is illuminated for a fraction of a second by transmitted light from a lamp placed behind the stand, the duration of the exposure being regulated by the shutter."

In the "Spot-Pattern" test the cards were of different nature. An irregular but carefully prepared pattern was made by piercing from four to eight large pin-holes in opaque squares of cardboard. Over the surface to be presented to the boys was pasted a square (1½ x 1½ inches) of moderately thick paper ruled into 36 smaller squares.

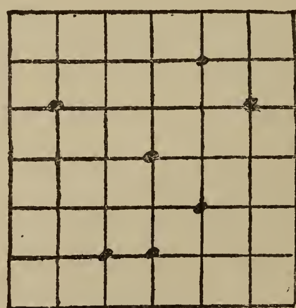


Fig. IX.

When the tachistoscope was open the pattern made by the light shining through the holes in the card and through the paper fell entirely within the larger square, each spot of light falling directly on one of the 25 inside corners. The subject was provided with sectional paper which duplicated precisely that used to cover the pattern. A sample card with nine spots was inserted and the shutter held open as for a time exposure. To avoid after-images,

the room was kept dusky rather than dark. The following instructions were given:

"Do you see those spots? Well, notice how they are arranged. Each one falls on a corner. And the card is ruled like this page, isn't it? With another card something like this, I am going to show you some light spots and I want you to copy the pattern from memory. You'll not get to look at them long, but in 3 short flashes like this. [Illustrate, counting the flashes aloud.] After the third flash I want you to put a cross on the corners here [on the sectional paper] corresponding to the corners here [on the card]. That is, if there should be a spot of light here, you should put a cross here [pointing in both cases]. If there, then there. You must get them on the exact corner as nearly as you can. Now do you understand? You wait till after the third flash and then copy down the whole pattern as soon as you can, so as not to forget it. This first card has only 4 spots. Start with your eyes looking at the center here. Ready? One—two—three" (opening the shutter just after each count). The exposure was about 1-10 of a second, the interval between exposures was as near 2 seconds as possible. Mistakes on the first card were pointed out and the subject urged to greater effort and attentiveness. "There will be four spots again this time. You ought to get them *all* right." There were two cards for each number of spots—from four to eight inclusive. Generally it was found wise to explain also the mechanism of the tachistoscope to the boys as several were burning with curiosity to know how the flashes were produced.

Important changes in procedure from that adopted by Burt (2) will be noticed. These were introduced primarily to save time, as he found the test very long and tedious, the number of required flashes sometimes being as high as 150. In the present form, 30 flashes are given for each subject. This is sufficiently tedious. About a month intervened between the first and second testings. One point was scored for each spot correctly located.

The reliability of this test is exactly the same in the two schools, r = .412. The correlations with other tests present only two noteworthy features (Table XVI). There is a markedly

lower correlation between all parts of the "Alphabet" test and the "Spot Pattern" in Group B than in Group A. Except for the correlation of "Spot Pattern" with "Alternating," however, this is more apparent than real, since the differences lie within the margin of their probable errors. It is somewhat difficult to conjecture what should be the ground of a rather high correlation in one group, this correlation being almost lacking in the other. Exactly the same point is raised in connection with the three memory tests. No manner of juggling can make a correlation of .064 ("Spot Pattern" with "Immediate Memory," Group B) a possible equal of one of .496 ± .088 (Group A).[15] In Group B the correlation with "Imputed Intelligence" is very low, almost negligible; in Group A the correlation is rather high. One is very sorry at this point that the method of provisionally estimating intelligence in the latter case is so crude. As it is, all that one dares conclude is that the test shows a small but positive correlation with intelligence.

TABLE IX

"Spot Pattern"—Results in No. of Spots Located

	Averages by Divisions											
	I	II	III	IV	V	VI	Best	Worst	Median	Mean	s	E of M
Group A	19.17	22.67	26.	29.	31.17	36.	43	17	20	27.32	5.37	±.596
Group B	18.	20.4	23.	24.4	27.	34.	37	15	24	24.23	5.62	±.68
Second Testing												
Group A	19.7	24.	26.	28.7	31.5	35.	37	14	27	27.46	5.33	±.59
Group B	17.4	21.2	23.4	26.6	29.8	32.6	36	15	25	25.13	5.62	±.68
Amalgamation												
Group A	19.37	23.33	26.	28.85	31.33	35.5	40	20.25	27.75	27.39	5.76	±.64
Group B	17.7	20.8	23.2	25.5	28.4	33.3	35.5	18.5	24.5	24.68	5.56	±.67

As will be clearly seen from Table IX and Fig. X, the difference between the two groups is not great, although the superiority of Group A is well beyond the accidental. With another system of marking this would be slightly greater, for it is obvious

[15] It is interesting to note that Burt got the same anomalous result. In Group B the correlation of "Memory" with "Spot Pattern" was .26 ± .11, in Group A .74 ± .09. While Burt's technique was different, it seems likely that the same function was tested. He gets fairly high correlations with estimated intelligence at both schools.

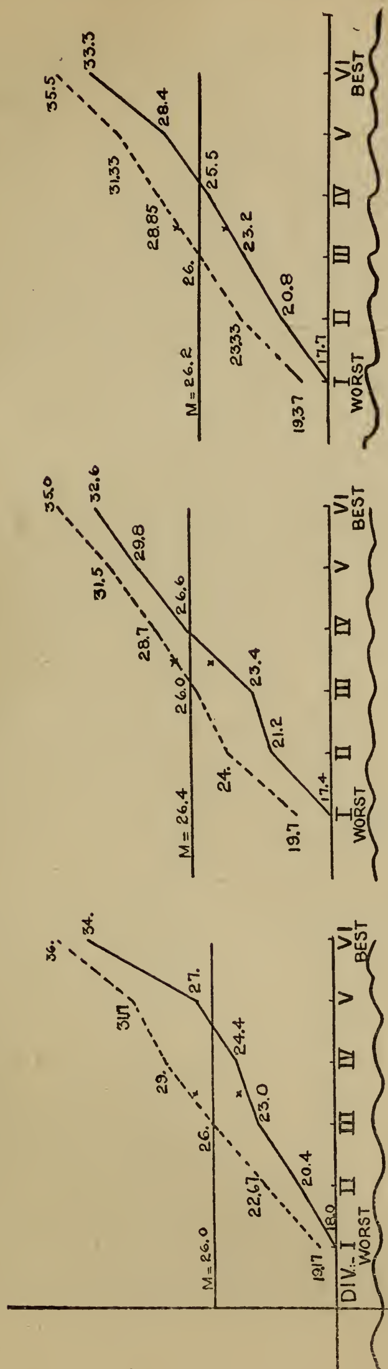

M = 26.2
M = 26.4
M = 26.0
DIV.: I WORST
II
III
IV
V
VI BEST
35.5
33.3
28.4
31.33
25.5
28.85
23.2
26.
20.8
23.33
17.7
19.37
35.0
32.6
29.8
31.5
26.6
28.7
23.4
26.0
21.2
24.
17.4
19.7
36.
34.
27.
31.17
24.4
29.
23.0
26.
20.4
22.67
18.0
19.17

Fig. X.

that the scoring was rather crude. A subject might learn the pattern quite correctly but locate it a square to one side, thus perhaps getting none right. Another subject simply puts down the requisite number of points at random and his chances of getting some one or two right are good. It seemed unwise, none the less, to introduce a more complicated scoring. It is exceedingly laborious. One's judgment is involved and this is notoriously not a static thing. And finally there is the doubtful or rather low correlation of the list with intelligence.

The test aims at testing a very useful function or group of functions of intelligence—chiefly range of attention and the ability to apprehend and remember relationships—at least spatial relationships. The question is, does the test measure these functions? And to this one must answer "only partially." The combining of attention with the relation-grasping function is rather unfortunate and on the whole unnecessary. The element of luck is rather large (although the reliability coefficient of .41 would seem to indicate that it was not so large as it had seemed to the writer in giving and in marking the test) and can not be eliminated. It seems to the writer that one would do better to use separate tests for attention (there are several pretty good ones) and for relations of various kinds. The portable tachistoscope is very handy and may well be retained for many of the simpler tachistoscopic experiments or tests.

9. "Related Memory" or "Memory for Related Words" test

It is thought that the test is a new one, although it resembles many other memory tests. No apparatus beyond a stop-watch is required. The subject is instructed as follows: "I am going to read you a list of words and when I've finished I'll give you the first one and you are to repeat the rest of the words in the order I read them to you. All these words have some sort of connection with each other. For instance, suppose I read 'eyes—blue—red.' Eyes are connected with blue because they often *are* blue, and blue with red because they are both *colors*. But this is not to say there's a connection between eyes and red. Each word is connected with the word going before and the word coming after it. And as a little 'tip' let me say that the better you notice

the connections, the better you'll remember the words. So notice these connections while I read slowly. Then I'll tell you the first word and you go right through the list. If you make a mistake, I'll correct you, e. g., if you say 'eyes—red,' I'll say, 'No, eyes?' and wait for you to give blue, i. e., I'll repeat the last correct word you said. If you can't think of a word in time, I'll prompt you—and of course score it against you. Now are you quite sure you understand what you are to do?"

One of the following lists of 21 words was then read at the rate of 1 in about 2½ seconds. The tempo was kept fairly constant by repeating each word silently before reading the next, as well as by watching the stop-watch. List 1 is read for the first testing, list 2 for the second and list 3 is kept in reserve—since it frequently happens that a test is spoiled by interruptions, misunderstanding, etc. It is thought that the 3 lists are of fairly equal difficulty, though much depends upon the accidents of training. It will be noticed that some of the words are equivocally used. This is intentional as it makes the task a little harder and thus gives more scope for intelligence.

List 1	List 2	List 3
bird	top	nut
nest	spin	crack
bush	spider	cracker
garden	fly	Christmas
house	run	holly
roof	cricket	prick
chimney	bat	pin
fire	night	steel
heat	cold	knife
boil	marble	cut
milk	round	cake
cow	ring	current
grass	light	river
green	moon	water
color	man	rat
paint	policeman	white
dry	beat	pale
dust	stick	pink
march	glue	cream
hare	gum	yellow
red	tooth	sands

In recall, the subject is given 10 seconds for each word. If he fails to recall it in that time he is prompted and told to continue. Corrections were made as described in the instructions.

If, when a subject is corrected for the proleptic insertion of a word actually in the list, he recalls the right word within the 10 seconds, the score against him is only 1. Other insertions count 2, as do promptings.

The test was a reliable one, the coefficients of reliability being .446 ± .094 and .508 ± .104 for Groups A and B respectively. The other correlations (see Table XVI) exhibited by this test are very interesting and not a little anomalous. Take first Group B. The low correlation between this test and the "Spot Pattern" is a case in point. Both these tests involve the apprehension of relations, the one of spatial, the other of verbal or conceptual relations. Yet the correlation between the two in this group is just barely more than the probable error. The low correlation with "Imputed Intelligence" is another surprising and disappointing result. Its correlation with the order of the amalgamated tests is on the other hand very high.

Turning now to Group A, we find some curious changes. The correlation with the "Spot Pattern" becomes, as one would expect, moderately high. "Dotting," which gave a fairly high correlation with this test in Group B, here gives no correlation whatever. Except "Immediate Memory," "Tapping," "Dotting," and "Age," all the tests show in this group higher correlations with "Related Memory." Thus the few indications we have, including the fairly high correlation with "Imputed Intelligence," indicate that this test does correlate with intelligence in this group. One is somewhat at a loss to account for the divergencies between the two groups.[16]

[16] Miss Bickersteth and I did not practice this test together as much as the others and accordingly showed more individuality in applying it. Group A was first tested, followed closely by Group B. It so happens that while she tested the majority of the latter group, I tested the majority of the former.

Table X

Group B

1st Time Over,	2d Time Over,	Changes in Rank
Tested by M. B. 18	18	14 gain 4 lose
Tested by H. B. E. 13	M. B. 10	1 gains 1 no change 8 lose
	H. B. E. 3	3 lose

TABLE XI

"Related Memory"—Results in No. of errors
First Testing

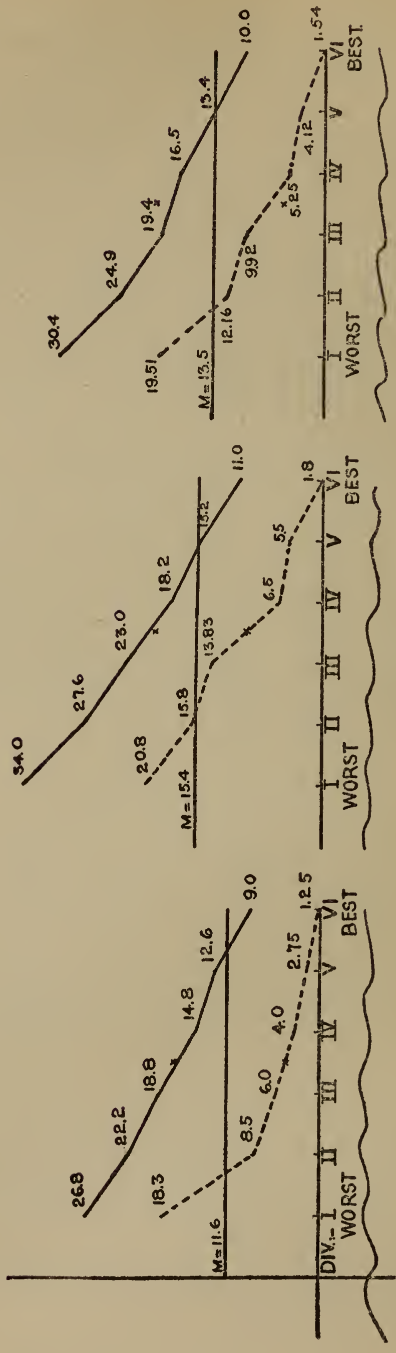
	Average by Divisions I	II	III	IV	V	VI	Best	Worst	Median	Mean	s	E of M
Group A	18.33	8.5	6.	4.	2.75	1.25	0	24	5	6.76	6.11	±.677
Group B	26.8	22.2	18.8	14.8	12.6	9.	4	30	17	17.35	6.53	±.791
Second Testing												
Group A	20.83	15.83	13.83	6.5	5.5	1.83	0	31	10	10.68	7.22	± .8
Group B	34	27.6	23.	18.2	15.2	11.	6	38	20	21.45	8.44	±1.022
Amalgamation												
Group A	19.56	12.16	9.92	5.25	4.13	1.54	0	22	6	8.72	6.53	± .724
Group B	30.4	24.9	19.4	16.5	13.4	10.	11	32	20.83	19.4	7.54	± .913

In no other test except "Narrative Memory" is the superiority of Group A so pronounced as in "Related Memory" (Fig. XI and Table XI). The worst (1st) division is seen to be as good at this test as the 3d division of Group B and the best division of Group B is not so good as the 3d best division of the rival group. That this is not due to the different experimenters is seen from the fact that it is greater in the first testing where Miss Bickersteth and I more evenly divided the groups than in the second testing.

TABLE X (continued)
Group A

1st Time Over,	2d Time Over,	Changes in Rank
M. B. 6	H. B. E. 3	2 gain 1 loses
	M. B. 3	2 gain 1 loses
H. B. E. 31	M. B. 9	3 gain 6 lose
	H. B. E. 22	2 no change 12 gain 2 lose

The interpretation of the above information is, however, far from easy. It might seem that the subjects found my peculiarities of method slightly the easier. If the test really correlates with intelligence in Group A (where I did most of the work) and if this greater ease is the cause, the section tested by me in Group B should exhibit higher correlation with intelligence than that tested by Miss Bickersteth. This is not the case. One safe inference we may draw—that personality and slight changes in method have an influence seemingly beyond all expectation—a point constantly to be borne in mind in working with tests.



30.4
24.9
19.4
16.5
13.4
10.0
19.51
12.16
9.92
5.25
4.12
1.54
M=13.5
I
II
III
IV
V
VI
WORST
BEST
34.0
27.6
23.0
18.2
13.2
11.0
20.8
15.8
13.83
6.5
5.5
1.8
M=15.4
26.8
22.2
18.8
14.8
12.6
9.0
18.3
8.5
6.0
4.0
2.75
1.25
M=11.6
DIV.:-

Fig. XI.

The test seems to us a very useful one and requires very little time and no apparatus. If younger or older subjects are to be treated, the number of words can be decreased or increased and the conceptual relations between the words made more or less obvious.

10. "Narrative Memory" test

The test is an old and familiar one, fully described by Whipple (9) who calls it "logical" memory. It was given by us as a group test. All subjects having been provided with paper, it was explained that a short story would be read to them. This story they were to remember and write down at once from memory. It was made clear that it would be read only once, that the exact words were perhaps advisable but not required and that, though ample time would be allowed, too much time should not be taken over the work. For the first story about five minutes were allowed for reproduction, although the experimenter used his judgment in allowing extensions to those of the younger subjects who were handicapped by slow penmanship. It was also explained that the story was Chinese, and the name of the principle character was written upon the board for them. The following story including the title was then read rather slowly to the pupils:

The Priest

"Tang, | when his | wife | died, | left home | and became a priest | of a particular order. | Some years afterwards | he returned | dressed in the garb | of his order | and carrying his praying mat | over his shoulder; | and after staying | one night | he wanted to go away again. | His friends however | would not give him back | his cassock | and staff; | so at length | he pretended | to take a stroll | outside the village, | and when there, | his clothes | and other belongings | came flying | out of the house | after him | and he got safely away." |

No. of "ideas," 31.

At the second test the following story was substituted for "The Priest."

The Spirits | of the Lake.

"An official | named Chung | was appointed | to a post | at Peking, | and on his way thither | crossed a lake. | Happening

to visit | the shrine | of the local spirits | he noticed an image | of a well known soldier | and another of a namesake of his own, | the latter occupying a very inferior position. | "Come! Come!" said Chung, | "my patron saint | shan't be put in the background | like that." | So he moved the image | into a more honourable place | and then | went | back | on board his boat again. | Soon after a great wind | struck the vessel | and carried away the mast | and sails; | at which the sailors | in great alarm | began to howl | and cry. | However, in a few moments | they saw a small skiff | come cutting through the waves | and before long | they were all | safely | on board.[17] | The man who rowed it | was strangely like | the image | in the shrine | the position of which Chung had changed. | He brought them to land safely | and then skiff | and man | both vanished." |

No. of "ideas," 46.

The stories used by Whipple (9), Henderson (4), Shaw (6) and others were rejected for several reasons. "The Marble Statue" is too well known, the paragraph on Cicero is rather abstruse and would certainly be more favorable to the boys at the Group A school who have studied classics, and "The Dutch Homestead" is too favorable to people with a good visual memory, whereas we were trying to test "logical" memory. The stories selected are simple, of a type not unfamiliar and yet with a flavor of novelty and almost certain not to have been previously read by any of the subjects. This test more nearly resembles school work than any other. In both schools, in fact, something very similar had been given as a class exercise, in neither case, however, at all frequently. It is possible though not likely that the overwhelming superiority of Group A is in part due to greater practice.

Marking this test is very difficult. The selections were divided into "ideas" or details as indicated above and subjects scored 1 for each idea reproduced. An appearance of having an objective standard is thus obtained. In application, however, one's personal judgment must play its part. What constitutes reproduction of an idea, how to grade a pupil with a charming concise style who gets the "meat" of a story with half the ideas reproduced by more verbose subjects—these are a few of the per-

[17] "They were all —— on board" is one idea, "safely" another.

plexities one meets. The writer corrected all the papers, going over them three times.

The reliability of the test is very high, the coefficients being, for Group A .587 ± .076, and for Group B, .556 ± .089. In both groups it correlates rather highly with "Imputed Intelligence." Of the other correlations one should perhaps notice in Group B the higher correlation of this test with all divisions of the "Alphabet test." None of the others seem to call for special comment here.

Table XII

"Narrative Memory"—Results in No. of units reproduced

First Testing

	Averages by Divisions						Highest possible score 31					
	I	II	III	IV	V	VI	Best	Worst	Median	Mean	s	E of M
Group A	12.16	17.83	19.0	21.16	22.0	25.16	27	8	20	19.6	4.43	±.492
Group B	9.8	10.8	12.8	15.6	17.0	19.4	20	9	14	17.8	5.40	±.654

Second Testing

							Highest possible score 46					
Group A	19.3	23.3	28.3	31.5	32.83	36.0	37	14	30	28.59	6.26	±.695
Group B	10.2	15.6	17.8	19.6	23.2	27.4	28	6	18	18.94	5.98	±.727

Amalgamation

							Highest possible score 38.15					
Group A	15.75	20.67	23.7	26.33	27.42	30.58	31.5	14.5	22.5	24.095	5.28	±.585
Group B	10.0	13.2	15.3	17.6	20.1	23.4	23.5	7.5	19.	18.37	5.20	±.630

As a reference to Fig. XII and Table XII will show, the superiority of Group A was almost overwhelming. The arbitrary abscissae chosen for the graphs rather obscure the fact that the top half of one curve merely reaches the bottom half of the other. When we observe, however, that the median of the one group is practically equal to the best attainment of the other, some measure of the superiority is gained. This is fairly evenly marked all along the line although, as is often the case, there is a tendency (here more pronounced than usual) for the poorest divisions to approach each other.

Less experience is required to give this than any other test and it is, therefore, more suitable for general application by teachers to large numbers of schools. After what was said above as to

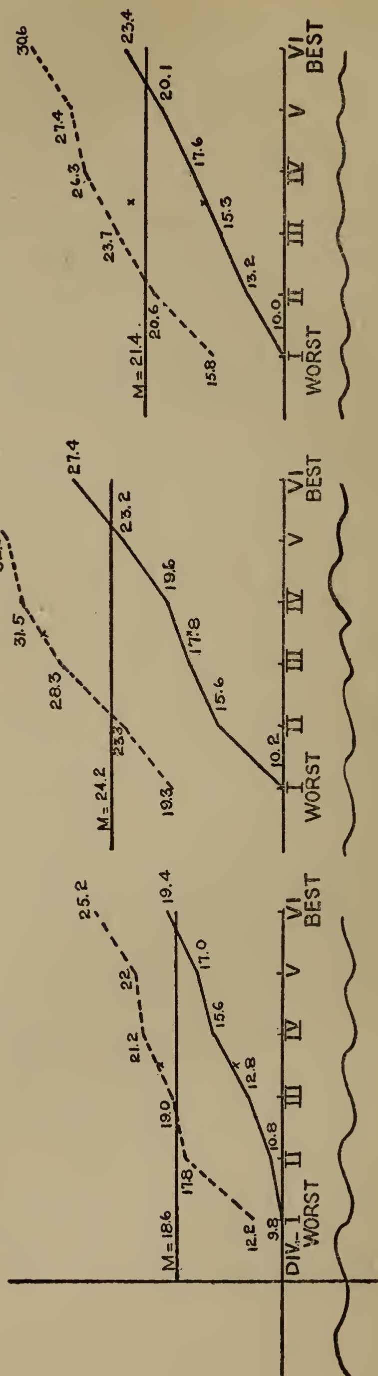

M=18.6
12.2
9.8
DIV.-I
WORST
17.8
II
10.8
19.0
III
12.8
21.2
IV
15.6
22
V
17.0
25.2
19.4
VI
BEST
M=24.2
19.3
23.3
I
WORST
10.2
II
28.3
15.6
III
31.5
17.8
IV
19.6
32.8
V
23.2
36.
27.4
VI
BEST
M=21.4
15.8
20.6
I
WORST
10.0
II
13.2
23.7
III
15.3
24.3
IV
17.6
27.4
V
20.1
30.6
23.4
VI
BEST

FIG. XII.

the difficulty of marking these papers, one need hardly point out that this should be done by *one person,* for the whole series of papers. To get more objective results, these might be separately marked by a second experimenter and the average taken. With these precautions, the test should be of very great and extended usefulness.

11. "Immediate Memory" or "Memory for Unrelated Words" test

The method is very like that of Meumann (3) as modified by Burt (2). The most convenient and simple form of apparatus is that devised by Miss Bickersteth. She fastened together three circular discs of mill-board in such a way that the inner disc could be rotated between the two outer discs. In these latter slots or windows were so cut as to show at a time only one of the words printed on the inner disc. By using both sides, each set of discs may be employed for two series of words. We actually used the same apparatus as Burt but are convinced that from the subject's view-point there is no difference between the two. The following five series of words were used, one of four words, three of six words each and one of eight words. One-syllable, common, concrete words were chosen but care was taken to secure that none had any very obvious connection with other words in the same or adjacent series.

First Testing

book	beg	cow	star	bell
mole	sun	sack	cork	kite
raid	joke	king	rice	sea
neck	rate	ride	man	mass
	meal	lake	pit	rut
	note	dog	lock	box
				maid
				gun

Second Testing

park	bat	snow	dell	chin
coat	tin	leg	pin	pig
shell	gate	cup	rose	desk
nail	rain	watch	frog	oar
	cut	pen	hut	nib
	fig	glove	cat	mass
				spoon
				fish

The instructions were as follows: "I am going to show you some short easy words through this slot. As they appear here you are to read them aloud. When you've read the last one, which will always have a line under it, you are to write them down here. And you must write them in the exact order in which they appear. Now I *don't* mean by that that you must write the first before you write the second and the third before the fourth. I mean that when you have finished, the first must stand here before the second. Write them *backwards* if you like, but then you must begin at the bottom. You see what I mean? Pay close attention while you read so that you can remember—and read them aloud! There will be four (six or eight) words this time. Ready?" As far as possible the words were presented to the subject so as to encourage him to read them in pairs with a marked rhythm but it must be confessed that none of the pupils took advantage of this to any marked degree. A few only of the better subjects in Group A read the words with a slight rhythm when repeating the test. Nevertheless in the preliminary test at St. Phillip's school, it was found that the series of 5 and of 7 were more difficult and hence only series of an even number of words were used. To urge subjects to read rhythmically only confuses them. A tablet with appropriately numbered spaces was provided for the writing.

It was felt that to score the same for words in a series of four and in a series of eight would be manifestly unfair. It is very easy to remember all the words in the shorter series, quite difficult to remember even the same number of words and get them into their proper places in an eight-word series. Accordingly it was decided to give 4 for each word in its proper position in the series of four, 6 for each word in the series of six, and 8 for each in the series of eight. For a displacement of one step in either direction, we deducted one point, of two steps, two points, and so on. A correct word moved from first to last place in any series thus counted one. If either the initial or final consonant or the medial vowel was incorrectly reproduced, the word was given half what it would score in that position. If more than one of these elements was wrong, the word was not considered reproduced at all. Fractional scores were not given.

The test is one of the most reliable used, the coefficients being .756 ± .050 and .508 ± .096 for Groups A and B respectively. The table of correlations shows few points of interest. One should perhaps note the high correlation with the "Spot Pattern" test in Group A and its failure to correlate with it in Group B. The explanation of this fact, however, lies rather in the fact that the "Spot Pattern" test exhibits different correlations in the two groups almost throughout. The higher correlation with "Analogies" and the very much higher correlation with "Imputed Intelligence" in Group A, leads one to judge that the test correlates with intelligence rather more highly in this group than in Group B. If we postulate (as the writer is strongly inclined to do) that the central factor in intelligence is adaptability, this becomes explicable. For it is quite certain that the instructions are too full to be easily grasped. In Group B fully 80% of the subjects had to be reminded that they were to read the words aloud. And it was quite clear that failure to grasp the idea of the test was so general that the whole factor of adaptability was somewhat obscured except where it was strongest.[18] The same considerations apply, it is true, to Group A, but to a very much smaller extent. To anticipate somewhat our general conclusion, the proportion of specially intelligent children is there undoubtedly (if our tests mean anything at all) very much greater. Hence the number of those whose power of adaptation would rise above the rather high threshold which the conditions of the test impose upon it, would be greater. And the more this plasticity comes into operation the higher the correlation with intelligence. Whether or not this somewhat speculative conclusion be valid, it is certain from our experience that the test would be improved if the instructions could be somewhat simplified. But the only way which we can suggest for this is a trial series of four.

Turning now to the actual results, we find once more a decisive and unequivocal superiority in Group A. A glance at Fig. XIII and Table XIII shows this quite plainly.

[18] The adaptability of one subject in Group A showed itself in an embarrassing manner. He adopted the plan of not reading the new word until he had run over the *previous* members of the series. Of course the scheme, however ingenious, could not be countenanced.

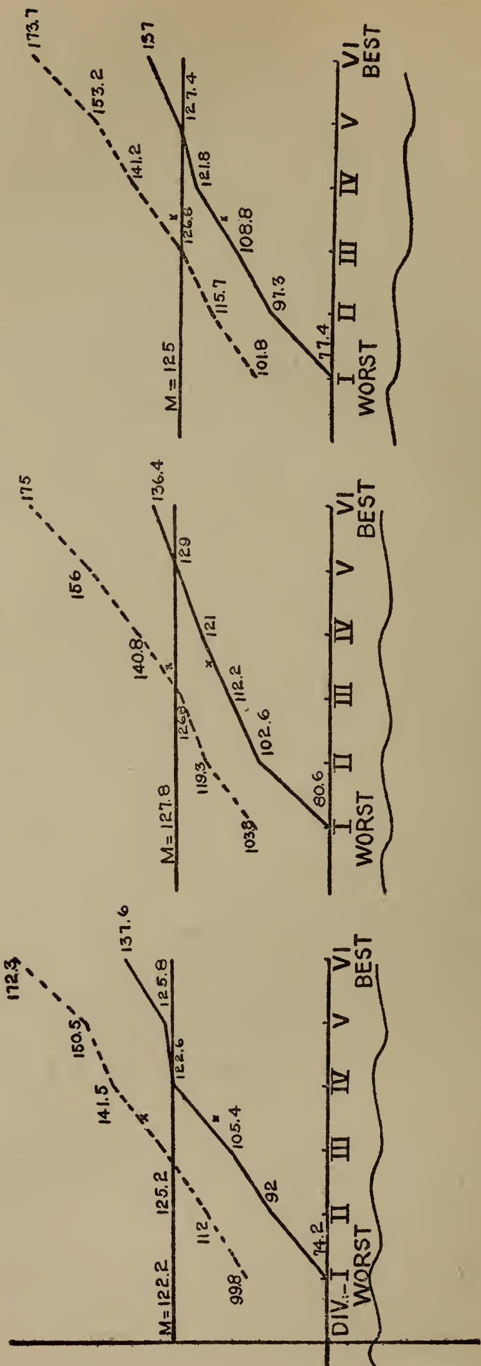


Fig. XIII

TABLE XIII

"Immediate Memory"—Results in units scored; possible score, 188.

	Averages by Divisions											
	I	II	III	IV	V	VI	Best	Worst	Median	Mean	s	E of M
First Testing												
Group A	99.8	112.0	125.2	141.5	150.5	172.3	188	97	133	133.81	36.68	±4.07
Group B	74.2	92.0	105.4	122.6	125.8	137.6	147	65	110	108.29	23.7	±2.87
Second Testing												
Group A	103.8	119.3	126.8	140.8	156.0	175.0	186	88	131	139.51	26.0	±2.89
Group B	80.6	102.6	112.2	121.0	129.0	136.4	141	63	118	113.77	18.99	±2.4
Amalgamation												
Group A	101.8	115.7	126.8	141.2	153.2	173.7	180.0	99.0	128.0	136.66	26.38	±2.93
Group B	77.4	97.3	108.8	121.8	127.4	137.0	137.5	64.5	112.5	111.03	21.83	±2.64

Additional evidence as to the value of this test is not needed, since it is perhaps the most extensively used single test. Our experience leads us to confirm Burt's view that simplicity and absence of distraction are more valuable than a perfectly regulated tempo. Miss Bickersteth is applying this as a group test. It will be interesting to compare her results with those obtained from Group B—her work being in comparable schools.

12, 13, 14. "Discs and Circles" tests

12 = "Disc Sorting;
13 = "Circle Judging";
14 = "Discs and Circles Combined."

The test is an entirely new one, designed to test three qualities: disc sorting for motor dexterity (a phase of discrimination reaction-time) and perceptual discrimination; circle judging for quickness and accuracy of discrimination—judgment; and both in combination for the ability to divide attention as well. This test was applied only to the boys of the Central school (Group B).

A reading desk or lectern was constructed by attaching to a substantial wooden base an upright, 28 cm. in height, and sufficiently broad and thick not to vibrate, which supported a rest at a convenient angle for reading (about 35 degrees from the perpendicular). The rest was provided with a small cleat at the bottom to keep the cards from slipping. In the same base just in front of the inclined lectern were set vertically, from left to right, three brass rods about 1 cm. in diameter, 30 cm. high, and 19 cm. apart. The tops of these rods were slightly rounded.

Thirty wooden discs were made, 15 white and 15 very dark brown, 5.5 cm. in diameter, 8 cm. thick and provided with a hole in the center about 1.3 cm. in diameter, so as to slip easily over the rods. The edges were milled and slightly beveled to facilitate grasping.

A small demonstration card had drawn upon it 12 pairs of circles ar-

ranged in 3 columns. In each pair, the circles (which were lettered) differed very materially in size. Three other cards had each thirty pairs of circles arranged in four columns. The circles here varied in diameter from 2.45—1.6 cm., but the difference in any one pair was never so great. The size-differences were of three grades of difficulty, with ten pairs for each grade. Unfortunately in the confusion involved in the commandeering of the Oxford Laboratory by the British War Office, these cards were lost and it is now impossible to say exactly what the differences were. They ranged from 1 mm. to 2 or 2.5 mm. The various size-differences were distributed in random order through the four columns of a card. As on the demonstration card, the circles were lettered. Only consonants were used and in each pair, consonants of distinctly different sounds—as X and T, or M and V.

The subject was seated before the apparatus on a rather high seat. The discs were put on the middle rod in the following order from bottom up: 1 white, 1 black, 2 white, 1 black, 1 white, 2 black and repeat. The order was kept the same for all, yet was sufficiently complicated to appear quite a random order to the subjects.

For "Disc Sorting," the subject was required to separate the discs, putting the white on the rod to the right, the black on the rod to the left, using only his right hand, taking the discs one at a time, and keeping his eye all the time on a fixation point in the middle of the lectern. Time was taken in fifths of a second.

For "Circle Judging," the subject was given a practice test on the demonstration card and the task carefully explained. He was to call the letter in the larger circle of each pair, taking each pair in order right down the column. Emphasis was laid as nearly as possible equally on correctness and speed. Time was taken as usual and the number of errors was noted. Different errors must be given different penalties since the difficulty of judgment is not equal.

For the combined test the subject was required to do both tasks simultaneously. It was made clear that the dropping of the disc need not be simultaneous with the giving of a judgment but the great majority of the subjects adopted this procedure—"from not being able to do anything else," they explained. The result was that the time of the combined tasks was that of the subject's slower task slightly increased. However, in a few cases there was a measureable difference between the times for the two tasks. Hence in all cases, the two tasks were separately timed, using a split-hand stop-watch. (One operation starts the two hands together but they can be separately stopped.)

The reliability for "Disc Sorting" is good ($r = .481 \pm .100$). The correlation with the headmaster's estimate of intelligence is about the same as that for "Dotting," $.262 \pm .120$. Other correlations may be found in the table. As might be expected, the correlation with "Dotting" is rather high but that with "Tapping" is unexpectedly low. None of the other correlations present unusual features except, perhaps, the divergence between the indices for "Narrative Memory" on the one hand, and that for "Related Memory" on the other. The best performance was 51.7″, the worst 67.9″, the median 57.4″.

The reliability is again high for "Circle Judging" (r = .565 ± .088) and the correlation with the headmaster's estimate is much greater (r = .315 ± .116). The best performance was 33 2/5", the worst 62 3/5", the median 52".

For the combined test, the subjects were ranked in order according to their performance in each of the two tasks and the two ranks then amalgamated. The reliability of such an amalgamated rank is always slightly higher than would appear from Spearman's "reliability coefficient," which in this case is already high (r = .565 ± .088. The correlation with "Imputed Intelligence" is not very great, but appreciable. The best performance was 70" for the discs, 51 2/5" for the circles, the poorest was 94" and 106 4/5", and the median was 73 4/5" and 76 1/5".

The test requires considerable practice for successful application and takes more than a quarter of an hour for each subject. This is probably more than the test as a whole is worth. The writer feels that only the judging of circles should be retained as a test of intelligence.

A possible improvement would be to put the pairs of circles on separate cards to be dealt out to the subject at a definite, regulated speed. Excellence would then be determined solely upon the number of errors, not upon a union of time and errors. Children, however, unless the time allowed for each judgment is long, are apt to get rattled and go all to pieces, calling out one or the other of the circles quite at random. If the time allowed is too long, the abler subjects can do the test too easily. Only by experiment could one tell whether or not the proposed change would work.

VII. General Conclusions

A. *The Usefulness of the Several Tests.* The "Alphabet" tests ("Letters," "Figures" and "Alternating"), "Analogies," and the three "Memory" tests, with perhaps the "Circle Judging" test are alone recommended for general purposes, and these should be modified in the manner described under their several headings. "Tapping," "Dotting," and "Disc Sorting" may be useful tests under more special conditions, though their correlation with "Intelligence" is probably negative or so low as to be somewhat doubtful. We found all of the tests more or less useful for our more specific purpose of comparing two groups of boys—each group homogeneous—of widely different inheritance.

Our testing was not, of course, as complete and adequate as one could wish, but even so, the time consumed for the ten tests was about one hour for each subject—for tests individually given—and half an hour more for the two group tests. The "Disc" and "Circle" tests increased the time by about twenty minutes

for each subject. We should have liked to test other functions, to test each function in two different ways, etc. But this time element, for both experimenters and subjects, is an inevitable bugbear in mental testing and one must draw what conclusions one can from what data the exigencies of the particular situation allow one to obtain.

Relatively speaking, then, memory (three tests) and perceptual discrimination ("Alphabet" tests and "Dotting" test) are here pretty thoroughly tested, and analogical reasoning ("Analogies") satisfactorily tested. So, too, with the function or group of functions enabling one to sustain a very rapid movement of the arm not requiring great accuracy ("Tapping"), and with the function or group of functions controlling accurate, quick movement ("Dotting"). Less satisfactorily and less directly we have tested attention (practically all tests but especially "Dotting," "Spot Pattern" and "Alphabet" tests) and the distributing of attention ("Alternating") and the ability to comprehend spacial relations or rather to introduce order into one's spacial perception ("Spot Pattern"). In the "Related Memory" and "Narrative Memory" tests, we have also an indirect measurement of the ability to grasp conceptual relationships of the simpler sort.

B. *Comparison of the Two Groups.*—One may first consider the extent of the superiority—already commented on—of Group A and then the distribution of the superiority within the groups. The material for the comparison lies in the tables and diagrams accompanying the respective tests and there discussed somewhat briefly. In many respects, the curves show most clearly and fairly how great is the superiority. Table XIV, however, enables one to compare the results with those of other experimenters. The percentage of each group falling below the amalgamated mean for all subjects, both groups, was calculated. As practice effects enter in, we take the two trials of the various tests separately.

It will be observed that about two-thirds of Group A and about one-third of Group B are above the mean. Since the "Tapping" test shows either no correlation with "Intelligence" or a negative one, it would be better to remove it from the list. We

TABLE XIV

Percentage of Subjects Falling Below the Mean

First Testing

	2. Letters	3. Figures	4. Alternating	5. Tapping	6. Analogies	7. Dotting	8. Spot Pattern	9. Related Memory	10. Narrative Memory	11. Immediate Memory	Averages
Amalgamated Mean of Two Groups	124.1	101.3	328.1	287.9	4.57	121.2	26.0	11.6	18.6	122.2	
% of Group A falling below this Mean	21.62	32.43	18.91	62.16	10.81	57.75	38.84	18.91	32.43	35.13	32.8
% of Group B falling below this Mean	54.84	58.06	58.06	38.71	48.39	61.29	67.74	74.20	83.88	70.97	61.61

Second Testing

	2. Letters	3. Figures	4. Alternating	5. Tapping	6. Analogies	7. Dotting	8. Spot Pattern	9. Related Memory	10. Narrative Memory	11. Immediate Memory	Averages
Amalgamated Mean of Two Groups	113.5	98.2	289.3	281.5	6.12	132.3	26.4	15.4	24.2	127.8	
% of Group A falling below Mean	37.84	35.13	16.21	56.75	24.32	45.94	45.94	18.91	27.03	40.54	34.86
% of Group B falling below Mean	54.84	70.97	67.74	32.26	64.52	58.06	61.29	70.97	74.20	70.97	62.58

Average of Two Testings

	2. Letters	3. Figures	4. Alternating	5. Tapping	6. Analogies	7. Dotting	8. Spot Pattern	9. Related Memory	10. Narrative Memory	11. Immediate Memory	Averages
Amalgamated Mean of Two Groups	118.8	99.8	308.7	284.8	5.34	126.8	26.2	13.5	21.4	125.0	
% of Group A falling below Mean	29.73	33.78	17.56	59.43	17.56	51.34	41.89	18.91	29.73	37.84	33.83
% of Group B falling below Mean	54.84	64.52	62.9	35.48	56.46	59.68	64.52	72.58	79.04	70.97	62.1

TABLE XV

First Testing

Percentage of Group B Surpassing	2. Letters	3. Figures	4. Alternating	5. Tapping	6. Analogies	7. Dotting	8. Spot Pattern	9. Related Memory	10. Narrative Memory	11. Immediate Memory	Average	Average Without Tapping
All but 1, or 96.2% of Group A	0.	0.	0.	9.68	3.23	6.45	0.	0.	0.	0.	1.94	1.07
All but 4, or 89.2% of Group A	9.68	0.	0.	22.58	3.23	9.68	3.23	0.	0.	0.	4.84	2.87
All but 9, or 75.7% of Group A	9.68	3.23	0.	41.94	6.45	29.03	16.13	0.	0.	3.23	10.97	7.53
50% of Group A	22.58	9.68	3.23	70.97	6.45	45.16	19.35	3.23	0.	9.68	19.03	13.29
Lowest 9, or 24.3% of Group A	38.71	61.29	38.71	90.33	29.03	64.52	64.52	3.23	16.13	41.94	44.84	39.79
Lowest 4, or 10.8% of Group A	67.74	74.20	64.52	100.	74.20	77.42	74.20	41.94	38.71	67.74	68.07	64.52
Lowest 2, or 5.4% of Group A	77.42	87.10	93.55	100.	96.78	87.10	90.33	71.	100.	70.97	87.42	86.02
Lowest 1, or 2.7% of Group A	77.42	87.10	96.78	100.	96.78	93.55	96.78	83.9	100.	70.97	89.33	88.14

Second Testing

Percentage of Group B Surpassing	2. Letters	3. Figures	4. Alternating	5. Tapping	6. Analogies	7. Dotting	8. Spot Pattern	9. Related Memory	10. Narrative Memory	11. Immediate Memory	Average	Average Without Tapping
All but 1, or 96.2% of Group A	0.	3.23	0.	6.45	3.23	9.68	0.	0.	0.	0.	2.26	1.78
All but 4, or 89.2% of Group A	0.	6.45	0.	16.13	3.23	12.90	3.23	0.	0.	0.	4.19	2.87
All but 9, or 75.7% of Group A	9.68	9.68	0.	48.39	3.23	29.03	12.90	0.	0.	0.	10.59	6.17
50% of Group A	19.35	19.35	22.58	80.65	19.35	35.48	32.26	3.23	0.	12.90	24.52	18.28
Lowest 9, or 24.3% of Group A	64.52	35.48	32.26	96.78	41.94	77.42	51.61	25.81	25.81	48.39	50.	47.03
Lowest 4, or 10.8% of Group A	74.20	45.16	64.52	100.	64.52	83.88	77.42	45.16	32.26	64.52	65.16	61.28
Lowest 2, or 5.4% of Group A	87.10	64.52	74.20	100.	77.42	96.78	80.65	48.39	41.94	79.42	74.84	72.05
Lowest 1, or 2.7% of Group A	93.55	70.97	80.65	100.	83.88	96.78	100.	90.33	83.88	87.10	88.71	87.36

TABLE XV Continued

Average of Two Testings

Percentage of Group B Surpassing	2. Letters	3. Figures	4. Alternating	5. Tapping	6. Analogies	7. Dotting	8. Spot Pattern	9. Related Memory	10. Narrative Memory	11. Immediate Memory	Average	Average Without Tapping
All but 1, or 96.2% of Group A	0.	1.62	0.	8.06	3.23	8.06	0.	0.	0.	0.	2.10	1.43
All but 4, or 89.2% of Group A	4.84	3.23	0.	19.36	3.23	11.29	3.23	0.	0.	0.	4.52	2.87
All but 9, or 75.7% of Group A	9.68	6.46	0.	45.16	4.84	29.03	14.52	0.	0.	1.62	10.78	6.85
50% of Group A	20.96	14.52	12.90	75.81	12.90	40.32	25.80	3.23	0.	11.29	21.78	15.78
Lowest 9, or 24.3% of Group A	51.62	48.38	35.48	93.56	35.48	70.97	58.06	14.52	20.97	45.16	47.42	43.41
Lowest 4, or 10.8% of Group A	70.97	59.68	64.52	100.	69.36	80.65	75.81	23.55	35.48	66.13	66.62	62.90
Lowest 2, or 5.4% of Group A	82.26	75.81	83.88	100.	87.10	91.94	85.49	59.68	70.97	74.20	81.13	79.04
Lowest 1, or 2.7% of Group A	85.48	79.04	88.72	100.	90.33	95.16	98.39	87.10	91.94	79.04	89.02	87.75

then find the proportion even more in favor of Group A. An average of but 30.95% of the group falls below the mean while in the rival group an average of 65.0% falls below, or more than twice as many. The extent to which the two groups overlap may also be judged from Table XV showing the percentage of those in Group B surpassing certain given percents. of Group A.

These figures show, then, a quite unambiguous superiority in the boys drawn from the better social class. Let us inquire a little more in detail into the distribution of this superiority among the members of the respective groups. Both groups show a few individuals who are very stupid relative to the other members of their group. They are sharply marked off even from those subjects just next above them. The number of these very poor subjects varies, of course, from test to test, but is usually from two to four. If stupidity be measured by the attainment of the stupid members of the better group, we have two to four stupid subjects in that group and seven to twelve in the other—from three to four times as large a proportion. If, on the other hand, stupidity be measured by the attainments of the stupid members of the poorer group, we have four stupid subjects there and but one in the better group.

The next seven boys in Group A are what one must call rather dull. Eleven pupils in the other school have similar attainments —about twice as many in proportion. Less than half the boys in Group B rise *above* the attainment of the "dull" members of the other group. The next nineteen boys in Group A represent the average for that group, constituting a little over half of the total number. Even this average attainment was too much for the other group, only 37% doing as well.

The superiority is still more marked when we come to the "bright" class. As compared with 22% in the A school, only 6% in the B school reach this standard. Finally, in only two tests out of nine ("Tapping" being omitted in all of the above, since it does not correlate with "Intelligence") the best subject was from Group B, in seven from Group A. In the "Dotting" test, the second best subject was in Group B, in all the others he was in Group A.

The following distribution curve presents these facts in graphic form.[19]

The difference between the two groups is marked at every point. The proportion in the one group of very inferior and in-

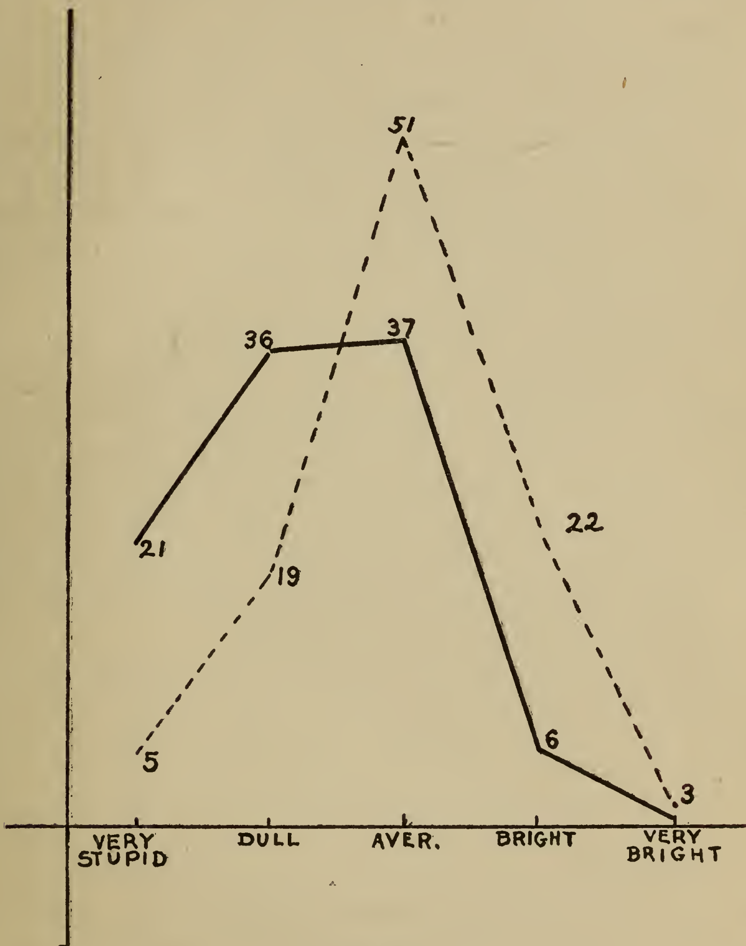


FIG. XIV

[19] The division into stupid, dull, average, bright and very bright is, of course, largely arbitrary. The writer merely classed the Group A boys according to their attainments in the tests roughly into those categories and placed in any given category those members of the other group having similar attainments. Thus whatever be the validity of the terms applied, the comparison is fair.

ferior subjects is so large that not even the number of average or mediocre subjects compares favorably with that of the other group. In the "bright" and "very bright" categories, the comparison is a little more favorable. For although but two subjects reach this category in Group B as compared with nine in Group A, yet these two do pretty well, since they rank fourth and sixth for all tests together, and all subjects. Yet here, too, it is clear that the superiority is unqualifiedly on the side of Group A.

Now, although the groups are small, they are exceedingly homogeneous and thoroughly representative of the children in two social or economic strata. The writer does not hesitate, therefore, to predicate these results for the children of the entire classes represented, or to conclude that the children of the professional class exhibit between 12 and 14 years of age a very marked superiority in intelligence—in so far as our results give a picture of intelligence. How much of the superiority are we justified in ascribing to heredity and how much to environment? This is quite a different question from that of *untrained* versus *trained* functions. Speaking broadly, all of these tests levy upon functions which have been exercised. But specific training is a different matter. With the exception of "Narrative Memory" the subjects had no specific training for any of the tests. In that test, all had had some specific training.

At the very outset we are confronted by one striking and constant difference (already mentioned) in the attitudes of the two groups—a difference in alertness. The boys in Group A seemed somehow more vital, more vibrantly, exuberantly alive than those of Group B. This trait shows itself in the tests in various forms: in greater energy, in a keener sense of competition, perhaps in better attention, and in a greater intellectual curiosity. Certainly this general alertness (or its absence) affected the quality of performance. I speak now of groups, not of individuals, for there were conspicuous exceptions in Group B, less conspicuous exceptions in Group A. How much of this, then, may we attribute to environment? None at all to the influence of the teachers, for the whole attitude of the teachers of Group B was far less repressive, less rigorously disciplinarian than of the

Group A teachers. It is probably true that discipline was less in evidence in the one school than in the other largely because less needed—and this, perhaps, because of the natively greater "docility" of the Group B pupils.

A certain amount of the difference between the groups is no doubt due to home influences. Almost as important is the influence of fellow pupils. In an atmosphere of mental alertness, the duller boys are stimulated to more than their normal energy. That the majority of the Group A boys were more alert is, in the writer's opinion, responsible for the fact that there were *no* cases in this group of the dull, passive type. It does not follow, however, that in an inert group, native alertness is stifled. On the contrary, it is developed. Leadership, with its responsibility, is very valuable and naturally falls to the lot of wide-awake members of the group. And it is to this we attribute the fact that a few of the boys of Group B are as quick and as energetic as all save the best boys of Group A, although the majority are, as above stated, somehow less "vital." This trait, then, of alertness may be due in part to environment. Yet that cannot be the sole explanation. Some part, in the author's opinion, a very large part, is due to heredity—certain features in the environment of both schools simply helping to develop the trait if one has it inborn. The influence of this characteristic is, in any case, very variable both with tests and with individuals. Generally, it is a help to its possessor, but, so far as one can see, not the chief determining factor. It is clear, at any rate, that the influence of environment upon the tests *via* "alertness" is small. Alertness, itself largely innate, is only one of many determining factors.

Again, if ability in these tests be due in large part to education and training, how are we to account for the very large individual differences within the two groups—differences greater than between the groups? For within these groups education has been almost exactly the same and even the home training has certainly been marked by great uniformity.

Unless, however, one has himself carried out the series of tests, the above observations may seem very speculative. We

may perhaps adduce a few more concrete bits of evidence. In groups as homogeneous as ours, age may be taken as a direct means of estimating the extent of the influence of environmental factors. It will be observed that the correlations of the several tests with age are very small, generally negligible, *especially in Group A where environment has been the better.* The smallness of the correlation of age and the several tests is a direct proof that environment and the tests have also a low correlation.

One may also refer to some results obtained by Mr. Cyril Burt (2). Working with tests, at least generally comparable with those above described, Burt found that after 18 months the same subjects showed either no marked improvement or an actual deterioration, most of the capacities remaining stationary. "Yet the boy's mental equipment has not. A somewhat 'dull' boy, for instance, who was 25th [out of 30] in the amalgamated list for Six Tests in 1908, has in 1909 risen to a place in the school occupied in 1908 by a 'clever' boy, who was then 4th on the amalgamated list; yet his new measurements instead of concomitantly rising to equal those of the 'clever' boy are now equivalent to those of the boy who was [then] 24th. Similarly with most of the boys. Thus, though the period between the ages of 13 and 15 is for boys one of rapid progress in knowledge, interests and acquired aptitudes, yet in the capacities measured by the tests no corresponding alteration is made. Hence, these capacities appear to constitute a relatively permanent endowment; and consequently it seems legitimate to assume that they depend upon innate differences in the individuals concerned" (pp. 175-176).

To this conclusion, the present writer subscribes. Although he is not prepared to say and does not in fact believe, that environment has had nothing to do with the superiority of one group over the other, he is convinced that *the hereditary factor plays an altogether preponderating part.*

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Table XVI

Table of Correlations

		2. Letters	3. Figures	4. Alternating	5. Tapping	6. Analogies	7. Dotting	8. Spot Pattern	9. Related Memory	10. Narrative Memory	11. Immediate Memory	12. Disc Sorting	13. Circle Judging	14. Discs & Circles	15. Age	16. Amalgted. Tests	17. Imputed Intelligence	Test No.
2. Letters	Group A		47±09	74±05	02±12	26±11	06±12	33±10	46±09	26±11	31±11				13±12	73±05	25±11	2.
	Group B		72±06	75±06	02±13	38±11	43±10	24±12	43±10	59±08	53±09	18±12	52±09	31±12	47±10	87±03	28±12	
3. Figures	Group A	47±09		45±09	—11±12	07±12	—11±12	21±11	36±10	24±11	30±11				24±11	57±08	32±10	3.
	Group B	72±06		81±04	—16±13	07±13	29±12	14±13	32±12	62±08	57±09	18±12	54±09	40±11	33±12	69±07	003	
4. Alternating	Group A	74±05	45±09		—11±12	35±10	—01±12	42±10	47±09	33±10	50±09				13±12	80±04	58±08	4.
	Group B	75±06	81±05		03±13	37±11	25±12	17±12	36±11	64±08	61±08	18±12	53±09	41±11	29±12	82±04	30±12	
5. Tapping	Group A	02±12	—11±12	—11±12		—52±08	58±08	—24±11	—24±11	—40±10	—57±08				10±12	—01±12	—39±10	5.
	Group B	02±13	—16±13	03±13		—10±13	22±12	19±12	001	—27±12	16±13	12±13	26±12	06±13	25±12	15±13	03±13	
6. Analogies	Group A	26±11	07±12	35±10	—52±08		—52±08	28±11	53±08	51±09	66±07				01±12	55±09	41±10	6.
	Group B	38±11	07±13	37±11	—10±13		08±13	35±11	26±12	56±09	29±12	24±12	27±12	15±13	02±13	55±09	68±07	
7. Dotting	Group A	06±12	—11±12	—01±12	58±08	—52±08		05±12	—04±12	—21±11	—47±09				13±12	08±12	—23±11	7.
	Group B	43±10	29±12	25±12	22±12	08±13		02±13	37±11	10±13	04±13	54±09	51±09	35±11	28±12	45±10	22±12	
8. Spot Pattern	Group A	33±10	21±11	42±10	—24±11	28±11	05±12		30±11	50±09	50±09				18±11	50±09	38±10	8.
	Group B	24±12	14±13	17±12	19±12	35±11	02±13		15±13	32±12	06±13	40±10	32±12	39±11	05±13	45±10	12±13	
9. Related	Group A	46±09	36±10	47±09	—24±11	53±08	—04±12	30±11		43±10	41±10				06±12	71±06	31±11	9.
Memory	Group B	43±10	32±12	36±11	001	26±12	37±11	15±13		37±11	48±10	33±12	18±12	—05±13	30±12	62±07	17±12	
10. Narrative	Group A	26±11	24±11	33±10	—40±10	51±09	—21±11	50±09	43±10		50±09				08±12	58±08	56±08	10.
Memory	Group B	59±08	62±08	64±08	—27±12	56±09	10±13	32±12	37±11		41±11	05±13	16±12	13±11	14±13	71±06	45±10	
11. Immediate	Group A	31±11	30±11	50±09	—57±08	66±07	—47±09	50±09	41±10	50±09					32±11	62±07	73±06	11.
Memory	Group B	53±09	57±09	61±08	16±13	29±12	04±13	06±13	48±10	41±11		11±13	32±12	32±12	24±12	66±07	49±10	
12. Disc Sorting	Group A																	12.
	Group B	18±12	18±12	18±12	12±13	24±12	54±09	40±10	33±12	05±13	11±13		21±12	23±12	10±13		26±12	
13. Circle Judging	Group A																	13.
	Group B	52±09	54±09	53±09	26±12	27±12	51±09	32±12	18±12	16±12	32±12	21±12		72±06	35±11		32±12	
14. Discs & Circles	Group A																	14.
	Group B	31±12	40±11	41±11	06±13	15±13	35±11	39±11	—05±13	13±13	32±12	23±12	72±06		38±11		22±12	
15. Age	Group A	13±12	24±11	13±12	10±12	01±12	13±12	18±11	06±12	08±12	32±11					31±11	40±10	15.
	Group B	47±10	33±12	29±12	25±12	02±13	28±12	05±13	30±12	14±13	24±12	10±13	35±11	38±11		33±12	000	
16. Amalgamated	Group A	73±05	57±08	80±04	—01±12	55±08	08±12	50±09	71±06	58±08	62±07				31±11		61±07	16.
Tests	Group B	87±03	69±07	82±04	15±13	55±09	45±10	45±10	62±07	71±06	66±07				33±12		45±10	
17. Imputed	Group A	25±11	32±10	58±08	—39±10	41±10	—23±11	38±10	31±11	56±08	73±06				40±10	61±07		17.
Intelligence	Group B	28±12	003	30±12	03±13	68±07	22±12	12±13	17±12	45±10	49±10	26±12	32±12	22±12	000	45±10		
Test No.		2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	

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The Vertical-Horizontal Illusion

AN EXPERIMENTAL STUDY OF MERIDIONAL DISPARITIES IN THE VISUAL FIELD

By

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The writer is lastingly grateful to the faithful subjects named within, and especially to Dean James R. Angell and Professor Carr who served not only as subjects but as faithful, patient critics as well. Thanks are due also to Professor Lightner Witmer and to Messrs. Ginn & Company for the use of the cut on page 11.

Chicago, 1915.

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INTRODUCTION

The overestimation of a vertical distance when compared with a similar horizontal extent has been a topic of scientific discussion for more than sixty years.[1] It doubtless has been a familiar error of the school rooms from the beginning of geometrical drawings and long a matter of household familiarity with persons interested in the form and size of objects. To painters and architects the effects of this and similar illusions are well known. The psychologist is interested in these phenomena because of their connection with the entire problem of visual space perception.

The basis of this investigation is a comparison of eight radii of a circular field for the purpose of determining how much each may require to be lengthened or shortened in order that all shall appear equal. The right horizontal line is usually the norm of comparison. The subjective difference between it and an equal vertical line in the upper field is a type of the phenomenon above referred to and the starting point of this study. (See diagram, Figure 1.) There are three groups of experiments based upon the following questions: (1) Is the vertical-horizontal illusion an isolated phenomenon in the field of vision or one of a series of radial inequalities? (2) Are these, in turn, distinct in themselves, or connected with the still wider group of foveal-peripheral disparities? (3) What effect upon the magnitude of any or all of these phenomena will result from controlled variation of certain conditions assumed by various investigators as causal to the vertical-horizontal illusion? Finally, there remains the task of

Fig. 1.

[1] Apparently the first mention is by J. J. Oppel: Jahresbericht des physikalischen Vereins zu Frankfurt am Main, 1854-5, S. 37; 1856-7, S. 47; 1860-1, S. 26.

critically examining the evidence pointing to an anatomical basis for the illusions in question.

If the series of radial differences should be found to vary together, in response to the same changes of conditions, then it is safe to assume that all are a part of the same general phenomenon and that whatever theory is offered in explanation of the vertical-horizontal illusion must be sufficient in breadth to cover the entire series of disparities.

The experiment connects, then, with previous investigations not alone of the vertical-horizontal illusion, but with all those studies in which spacial estimation in one part of the visual field has been compared with that of any other part. These investigations are numerous and varied—extending over the past six decades—and show a wide variation as to both factual and theoretical conclusions. There is a difference of opinion as to whether we see the right field larger than the left, either with both eyes or with one; also, even, as to the comparative values of the upper and lower fields; but the agreement is practically universal that a vertical extent is estimated as greater than an objectively equal horizontal extent.

The theoretical explanations of the different investigators may be grouped conveniently according to two broad types of causes ascribed: *first,* asymmetries of the visual organ, whether of retinal formation, of eye curvature, or of muscular arrangement; *second,* erroneous central functioning, or misjudgments due to ideas of perspective, the influence of contour, contrast, or some more subtle idea entering into the perceptual interpretation. Combinations of cross threads from the two groups of explanatory material may be found; but in the main we are told, on the one hand, that the falsity is in the sense element solely, on the other, that the sense impression *per se* is perfect (that is, in correspondence with objective facts) and the error is one of judgment.

The array of contradictory results and opposed opinions invites sufficiently to further experimentation. There is a possibility that by a method similar to, but differing slightly from, those formerly used it may be shown that methodology has had

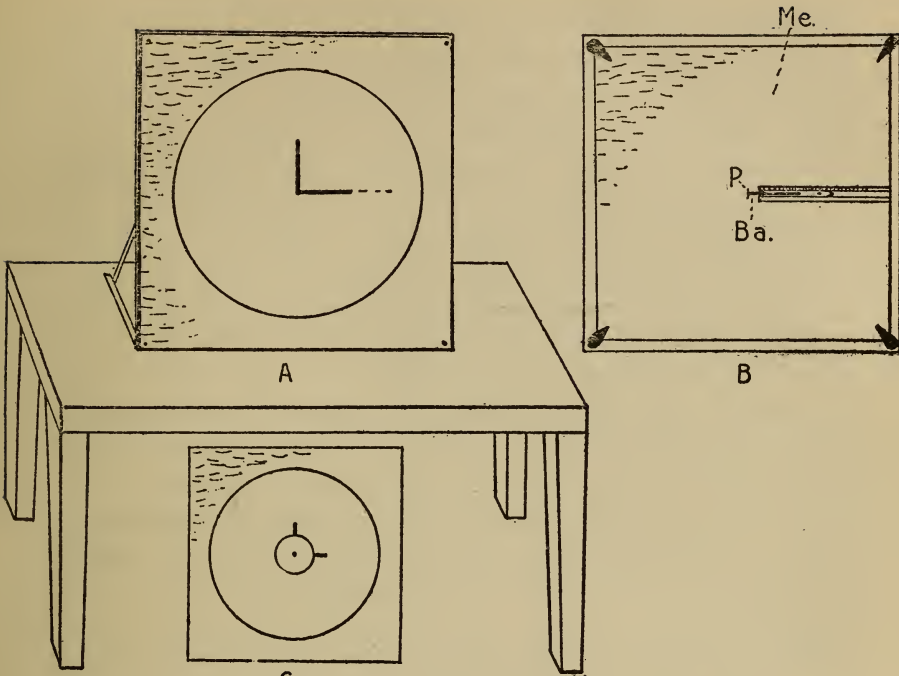


FIGURE 2.—A, front view of apparatus, with the vertical bar as the fixed standard and the horizontal (an extension of *Ba.* in Fig. B) as the variable line.—B, rear view, showing detachable metallic plate (Me.) in frame; also perforation (P) through which the bar (Ba.) passes to the front, to form the horizontal line at the right in A.—C, Same as A, with typical disc as used in Part II of the experiment.

something to do with the great variety of "facts" reported. Or, again, if it is found that by one method of procedure different subjects yield widely differing types of results, it may reasonably be inferred that a measure of the contradictions in reported "facts" is chargeable to individual peculiarities of the observers serving in the different investigations. None of these things could fail to assist in an analysis of existing theories or in pointing a way to a final understanding of the fundamental causes of certain of our illusions in visual space.

The definite aspects of the present problem will be stated in turn in connection with each experimental division.

Apparatus.—The final form of apparatus used was a device of Professor Carr's. (See Fig. 2.) A metal sheet, fixed in

an upright wooden frame 84 cm. square, supported a white background of cardboard, against which were exhibited two blackened steel bars 7 mm. in width. The "standard" bar was detachable. The other, or "variable," entered through a slit at the center from the rear of the metal sheet, where it was attached to a slide and, by means of the slide, to a Vernier scale the readings of which were accurate to 0.1 mm. The front of the apparatus was covered by a glass, fastened in the frame. This served to keep the cardboard and metal bars in a smooth, unified surface. The outline of the white field was modified by a gray cardboard mat, attached to the frame by thumb tacks. From the center of the mat any desired form was cut. Except where other figures are specified, the central field was uniformly a circle 68 cm. in diameter. The subject's head was supported by a mouth-bit rest. The apparatus stood upright upon a table, the center of the field being 129 cm. directly in front of the observer's eyes. The operator was concealed at the rear of the apparatus.

There were four possible positions for the adjustable line, or bar, namely, upon the right and left horizontal and the upper and lower vertical meridians. In the "norm" position, the operator found the Vernier scale and slide at her right hand (Fig. 2, B), and thus the variable bar was thrust through to what to the subject was the right horizontal line. (Fig. 2, A.) By turning the apparatus 90°, 180°, and 270° in the frame the other positions of the variable line were obtained. The meridional changes for the detachable standard had a wider range. A perforation at the end of the bar slipped over a small brass pin at the center of the field (just at the edge of the slit through which the adjustable entered). To the "free" end, on its under surface, was soldered a tiny metal point or pin which pricked into the cardboard and so held the bar in any desired meridian. For convenience the word meridian will be abbreviated to M. and the lines used will be named numerically, by their angular distance above or below the right horizontal, those below being primed. (See Fig. 3.) Thus, M 0 is the right horizontal meridian itself, M 180 is the line opposite, M 90′ is the lower

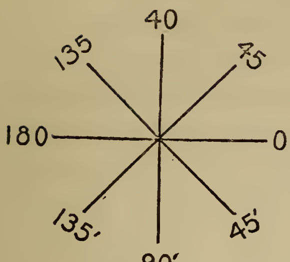


Fig. 3.

vertical, etc. Also for convenience the variable and standard bars will be called "lines," abbreviated to Var. and St., respectively, and referred to by their length. Thus "St. 160" or "line 160" will mean the standard *bar* of that length. In width these bars or lines were uniformly 7 mm., an arbitrary choice made because of the clear, distinct impression and the slight tax upon the attention.

The variable line had a range in length from a minimum of 10 mm. with the shortest standards to 295 mm. for the longest. The standard lines used ranged from 22.5 mm. to 260 mm. in length. That adopted as the normal was 140 mm. The visual angles subtended ranged from 1° 19′ for the shortest St. to 11° 24′ for the longest, and 6° 12′ for the normal St. The corresponding retinal images were as follows: shortest, .28 mm.; longest, 3.2 mm.; normal, 1.7 mm. The norms for length as well as width were chosen after preliminary tests.

Throughout the work of experimentation artificial lights were used, since they afforded greater constancy than could be expected from the sun. Two eight-candle power electric lights, with ground glass bulbs, screened in green, were placed one on each side of the subject and directed toward the apparatus. The room previously had been used for a dark room and all outside light was easily excluded.

Method.—Throughout the experiment there was one constant, apparently simple, problem for the observer, namely, to compare one fixed standard line with one variable line and to judge when the two were equal. It was therefore possible to have one general method of procedure. But the selection of that method was not a simple matter. There were the questions: Should the observer make the adjustments himself or not, and why? If the experimenter made them, should the method be that of "right and wrong cases" or of "minimal changes", and why? Each procedure offered some advantages, but notable disadvantages as well. A brief test of each was made in a preliminary series.

Errata

1. The upper vertical line of Fig. 3 should be marked 90, not 40.

The *method of minimal changes,* or least perceptible differences, had a promise of great accuracy, but proved tedious and taxing. Both the retina and the eye muscles fatigue readily with long fixation, and the judgment wavers with deliberation. Thus three possible causal factors were disturbed and complicated by the method itself. It is therefore not surprising that observers were confused and remarked that the entire figure seemed at times to change enormously in size. The *method of right and wrong* cases promised speed, but again carried its own illusions. The effect of contrast was so apparent, even with moderately small changes in the Var., that the operator gave up the plan, although the observer was unaware of the disturbing factors. There was left the *method of production,* or mean error. This would require less time than minimal changes and avoid irregularities of judgment due to such sudden contrasts as occurred with right and wrong cases. There were, however, aside from mechanical difficulties, these disadvantages in the subject's making his own adjustments—an added probability of his giving more direct attention to one line than to the other, and the further possibility that through the use of his hand the kinaesthetic space sense would unite with the visual estimations in forming his judgments. A *combination method* was finally decided upon, in which the process was that of *production* or *average error,* but with this alteration, namely, that the observer —who maintained fixation at the center of the field—dictated the adjustments, which were made by the operator.

The *order of procedure* in these adjustments may be made clear by taking a typical case, in which, for instance, the St. was at M 90 and the Var. at M o. The two-line figure was presented with the Var. at its minimum length for the given St. The subject, who had fixated the small brass pin at the point of juncture of the two lines, answered, for example, "Move out four centimeters," "Now one centimeter," "Now two millimeters," "Now a tiny bit more, less than a millimeter," "There!" If he found at any time that he had gone too far no record was made and a new start was taken. After each response, while waiting for the adjustment and the operator's "Ready," the subject

rested his eyes about 5 cm. below, or at the outside of, the angle. Ample time was allowed for a refixation in each case before a new adjustment was dictated. When the final judgment of equality was reached and recorded, the Var. was then pushed out somewhere near its maximum length and adjustments were similarly made in the reverse direction. From six to a dozen settings of the Var. were generally required in reaching a judgment of "equal." The subjects varied in this respect, some moving rapidly by long steps, others more slowly and deliberately. As a rule the rapid work was more consistent, though deliberation, *with method,* had fair success. As an example of the latter, subject Ca. stated when the work was completed that before giving a final judgment of equality in any case, he always closed his eyes for an instant and came back for a fresh look at the figure.

The adjustments continued, alternately, from greater to equality ("In" series) and from less to equality ("Out" series) until five judgments each way, or a total of ten for each pair of lines, were obtained. The order of presenting the Standards—supposing the Var. to be at M 0—was as follows: M 90, M 90′, M 45, M 45′, M 135, M 135′, M 180. (For other positions of the Var. the same angular relationship of the two lines was adhered to.) This complete circuit—once around the field in any given case—constitutes what will be termed a "single series." It was followed in every case by another "single series" in the reverse order of St. positions, and the two combined make what is termed a "double series." The averaging together of such a double series, as was done to obtain the majority of the tables and graphs, was for the purpose of eliminating the possibility of influence from the chance order of procedure upon estimations of successive lines, as well as to insure that a sufficient quantity of data entered into each average. In obtaining the data for the single series the judgments of the original "In" and "Out" series were first averaged separately and these results averaged together. (See Table VI.) This procedure made it possible, first, to scrutinize the effect of the direction of the adjustment, then in a measure to eliminate this influence—by the method of

averaging. Again, the double series with any one position of the Var. line was followed by such a series with each of the other possible positions of this line. By the averaging together of the four sets of data in which the influence of the adjustment factor was made to play against itself in two pairs of opposed directions, it was hoped to obtain a fair elimination of whatever disturbance arose from this source. Furthermore, an attempt was made from the beginning to forestall this influence of adjustment, or of objective movement, by having the judgments made only when the lines were stationary, by the subject removing his eyes while the adjustments were being made, and by his having ample time for a refixation after each signal of "Ready." Such are the main features in the order of procedure. Cues were avoided (1) by having different starting points for successive series in either direction, thereby breaking up the tendency to estimate "equality" by counting the "steps"; also (2) by the experimenter's being hidden and maintaining a monotone in giving signals. During rests conversation upon indifferent subjects was freely indulged. This was expected to interfere with memory cues and lead the subject to return for each judgment to the original sensory 'given.'

Certain features were standardized. In the procedure described above effort was made to preserve a uniformity in the observer's bodily and ocular position and in his attention attitude, since lack of control in these matters may be partially responsible for variations in the results of certain former investigations.—The subject was seated comfortably before a narrow table upon which the arms could rest. The table was fastened to the floor and to it was firmly attached the mouth-bit head rest. A constant position of the head aided in preserving a uniform balance of the ocular muscles, and, together with the unvarying fixation (with eyes in the "primary" position) of the center of the objective field, insured a fair uniformity of the retinal area explored. Furthermore, the subject was directed to attend mainly toward the standard, and report when the variable was too long or too short with respect to it. In brief, norms were established for all those conditions which were to be altered in the course of the experiment. Summarized, these norms are given below.

Normal Series.—(1) *Bodily Attitude:* Subject seated, eyes looking directly forward to center of apparatus; binocular fixation, eyes in the "primary" position; head position maintained by mouth-bit rest. (2) *Mental Attitude:* Attention directed mainly toward the St., the Var. to be estimated as too long or too short by comparison with it. (3) *The apparatus:* Circular field, radius 34 cm.; standard 140 mm. in length; order of presentation of standard—M 90, M 90′, M 45, M 45′, M 135, M 135′, M 180, then the reverse; position of the Var., M 0 (followed in "norm variation" series by the positions, M 180, M 90′, M 90); distance of the center of the field from the subject's eyes, 129 cm. (length of *retinal image* and size of *visual angle* formed by the normal St., 1.73 mm. and 6° 12′ respectively); (4) Lighting conditions of the field, uniform and constant.

Each of these conditions, with the exception of distance of the field from the eyes, and the meridional order of presentation of the St., was at some time in the course of the work made the central feature of investigation, and hence was varied singly while all other conditions remained constant. These variations will be described when the separate problems are discussed; but for all reference to a "normal" series, the above conditions are to be understood.

Perfection is not claimed for the method adopted. It is hoped that there is an elimination of the grosser errors involved in minimal changes and right and wrong cases, and a retention of the best features of the method of average error. All disadvantages of the latter are not ruled out; neither, it is believed, are they augmented in any way, while there is the one clear gain in not having the subject make his own adjustments. The defects of a method, if measurable in their effects and not destructive of the purpose of the experiment, may, in their turn, serve well in final interpretations. This is to be remarked of the influence of the adjustment factor, noticeable in this work and in that of all others who have used the method of mean error. If it is thought that even with the above precautions the prejudices of the operator may have influenced the results of her subjects

as a consequence of her having manipulated the apparatus, let it be said that none could be more surprised by the outcome of the experiment than the operator herself. The results, in fact, were not carefully evaluated until some time after the experiment was completed. There is sufficient individual variety, moreover, to guarantee that no one person's influence could have been responsible.

Subjects. The subjects serving regularly in this experiment were a professor and nine graduate students in the department of psychology. They were Professor H. A. Carr, Doctors W. S. Hunter, F. A. C. Perrin, R. B. Owens, Messrs. E. S. Jones, G. W. Kirn, J. O. Pyle, M. O. Beanblossom, Miss Katherine Taylor[2], and Mrs. L. A. Barr. All knew more or less of the vertical-horizontal illusion and that it was a feature of the experiment. None knew his results from day to day or the occasion of the special changes in the program. The instruction given to each was that he should endeavor to maintain a naïve attitude and seek to base his judgments upon original sense impressions and not upon deliberative estimations intended to balance an anticipated error. All subjects had had training in experimental psychology and hence realized the importance of fulfilling carefully the conditions. It is the writer's grateful opinion that no group would or could give greater effort to reporting faithfully what was *seen* or *sensed* rather than what was calculated to be true. However, two subjects, Be. and O., found it impossible to discard the central factors—the former measuring by the number of "steps," the latter by collapsing angles, etc. Their average variations were higher and their total results less consistent than those of any other subjects, and it was therefore necessary to substitute other observers in their places.

The total number of complete or abridged series served by each subject was as follows: Be., 21; O., 7; Ca., 34; Pe., 33; Ba., 54; Py., 64; Ki., 70; Ta., 34; Jo., 53; Hu., 62; a total of 28,540 judgments. This does not include preliminary tests made without apparatus nor certain supplementary series in which additional subjects (Po., S., A. R. C., T. J. C., J. R. A. and others) served for a brief time.

[2] Deceased.

The shape of the binocular field, because of the position of the eyes in the sockets, is objectively an oval. Whether this objective oval appears subjectively as such is a question. It might appear as a circle or as some other figure.[1] If it were objectively a real circle it might appear, again, as an oval. There is a possibility that the vertical-horizontal illusion may have either a correcting or a distorting effect upon the entire outline. But the whole field of vision can scarcely be subjected to the tests of this experiment. A limited portion, extending from the focus as far into the margin as practicable[2] in binocular vision, is here made the object of research. The determination of the subjective appearance of a real circle, based upon the estimations of the length of eight objectively equal radial lines, is the first problem with each observer. The subjective form outlined by the intercomparison of these radii, while the center is fixated and other conditions are "normal," is what is termed in this paper the "normal visual field."

The question may take one of two forms: first, what must be the actual character of a set of radiating lines or meridians that they may appear as radii of a circle, their common point being fixated; second, what is the subjective appearance of a set

[1] The approximate form of the actual figure when the entire field is attended to for experimental purposes is given by Witmer in the following figure. The problem of this paper falls well within the central area of binocular vision.

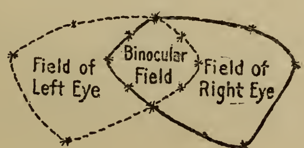


FIGURE 4—From Witmer's Analytical Psychology, p. 52. (Reduced.)

[2] The actual extent of the circular area was determined by the selection as the norm of a standard length which afforded the longest radii the subjects could grasp with any feeling of surety in estimations.

of equal radiating lines? The answer of the first aids in a determination of the second, which is the final object of the quest. The vertical-horizontal aspect of such a field is the matter of crucial interest in the investigation. Within the scope of this "normal" area are smaller concentric, or zonal, sections, centrally and marginally placed, which are likewise to be explored for typical aspects of the disparities found in the first survey. This makes up the second problem group. If for individuals or groups of individuals there develops through a long series of "normal" and "supplementary" tests a persistent *type field,* there remains finally the larger problem of explanation. This will occupy the third section of the experiments, and involve also a final critical examination of retinal conditions and a summary of theoretical data.

As indicated in the introduction, the literature of the earlier investigations would lead to great uncertainty of expectation in regard to the "field types," or outline forms. There is a fair agreement that the difference is greater between the vertical and horizontal dimensions than between any other lines of the field; otherwise, there is more diversity of opinion. According to Delboeuf[3] the elongation of the upper vertical is greater than that of the lower. If Fischer's[4] results are typical, the reverse is true.

Again, according to Kundt,[5] with binocular vision the right and left halves of the field should be nicely balanced, because each eye overestimates its outer field. Münsterberg[6] assures us that the left field is larger than the right, and that only under certain conditions is a vertical line exaggerated with respect to an horizontal. Stevens,[7] on the other hand, says not only is the upper vertical usually greater than both the lower vertical and the right and left horizontal dimensions, but the whole right field is phenomenally larger than the left; also that the periphery

[3] Bulletins de l'Académie Royale des Sciences de Belgique, 2, XIX, p. 195. 1865.

[4] Archiv für Ophthalmologie XXXVII, 1, S. 97-102; 3, S. 55. 1891.

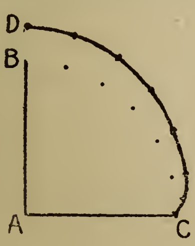
[5] Poggendorff's Annalen der Physik und Chemie, CXX, 118. 1863.

[6] Beiträge zur Experimentellen Psychologie, H. 1, S. 126. 1889.

[7] Psychological Review, Vol. XV, p. 69; Vol. XIX, p. 1.

is overestimated with respect to the foveal parts. Certain statements of James,[8] and also of Fischer, would indicate an opposite foveal-peripheral relationship. Wundt,[9] after years of investigation of his own and on the authority of others also, states that with monocular vision the outer or temporal field (viewed by the nasal side of the retina) is overestimated with respect to the inner field by about 1/40; that the upper vertical extent is overestimated with respect to the lower vertical by 1/16, and with respect to an equal horizontal line by about 1/7 to 1/10 or 1/20.

There is, however, an earlier piece of work by Wundt[10] that is of significance here. A double test was made in the following manner. First a standard distance of 20 mm. was marked off by two points upon a vertical, then upon a horizontal, line, and in each case a pair of compasses was so adjusted that one end rested upon one of the dots and the other was extended to an apparently equal distance with the standard but in line successively with the angular directions given below. Second, the standard was placed successively upon the other meridians and an equal extent was estimated (a) upon the vertical, then (b) upon the horizontal line. The data for the last named case is given below, and by extending each line the amount it was overestimated in terms of the horizontal the writer of this paper has cast the same into the form of a graph. (Fig. 5.)



Angular distance from the horizontal.	Horizontal estimated distances.
0°	20 mm.
15°	22
30°	22.5
45°	23.5
60°	24
75°	24.5
90°	25

Fig. 5.

A 90° rotation to the right or to the left may be necessary to convince the reader that the line CB of Figure 5 is an arc of a

[8] Psychology, Vol. II, p. 140.

[9] Die Geometrisch-optischen Täuschungen, S. 106. 1898. Outlines of Psychology (Judd), p. 136.

[10] Zeitschrift für Rationelle Medicin, 3 B. VII, S. 374. 1859; also Beiträge zur Theorie der Sinneswahrnehmung, S. 158. 1862.

circle. It must be borne in mind also that each of the indicated radii was compared singly with the horizontal standard; that is, the comparison of the lines was made in pairs, not in groups.—The meaning of the graph is that in Wundt's comparison of seven radii in a single quarter of a circular field the increasing overestimations of the lines as they approach the vertical inclination to the base produce subjectively a form resembling in the main a quarter of an upright oval, and the vertical itself is seen greater than the horizontal in the ratio of 25:20, or one-fourth larger. The bulging of the lines, however, begins *noticeably* with the first angular remove from the horizontal base, and in order that this segment of the field should appear as a quarter of a circle there would be required a gradual flattening, increasing toward the poles.

Granting that such should be the true outline of our subjective apprehension of a circular form, we would never be conscious of it in our ordinary vision. What we see (or estimate) as a circle on a plain wall before us may have radii of greatly varying objective lengths, but we do not know it. And in the drawing of a circle objective measurements and eye movements to various parts of the outline serve to correct our judgments. Wundt says the vertical-horizontal illusion is not apparent in the drawn circle; though others (e. g., Holtz[11]) say a circle drawn freehand is flattened at the poles, which would indicate that a real circle has subjectively a polar eccentricity similar in kind if not in degree to that of the above graph. The inference would be that the objective oval of our entire visual field, which is elongated horizontally, tends to bend itself toward the circular form. But Wundt did not extend his figure to so wide an application. And we are not at present concerned further with the inquiry as to the whole field, but rather with the factual relations within the scope of that area that is the basis of the present study.

PART I. TYPE FORMS: MERIDIONAL DISPARITIES

The first division of the tests is intended to answer the first of the questions mentioned in the introduction, i. e., what in the case of each observer is the subjective impression of a circular

[11] Wiedermann's Annalen der Physik und Chemie, X, S. 158. 1880.

form, as determined by the comparison, in pairs, of eight radii of this form? There are two sections: first, the "norm" series, with its variations; second, a supplementary series, intended to verify the results of the norm work and to add certain data of some intrinsic interest. All have reference to the general outline or *type form of the field* as controlled by subjective disparities in the radial lengths.

1. *The "Norm" Type; Norm Variations; and "Primary" Types*

The definite questions of this section are: (1) Given a standard line of 140 mm. at M 90, what will be the length of an apparently equal line at M 0? Given the same standard successively upon six other meridians (Fig. 3), what in each case will be the length of an apparently equal line at M 0? (2) By changing the position of the Var. successively from M 0 to M 180, M 90′, and M 90, and repeating the comparisons, what changes take place in the comparative lengths of the meridional extents? (3) Is there any harmony among all the series upon which may be based a conclusion as to the differences of spacial estimation in the meridians investigated; if so, what is the position of the vertical-horizontal illusion in the series of meridional differences; and what is the final form of the subjective field? Questions 1 and 2 are answered experimentally and the data given in the tables. The answer to the reverse of these questions —i. e., what is the apparent length of the St. in each case when the Var. is objectively equal thereto?—is approximated by transposing the added length from the Var. to the St., as is done in the graphs. This makes possible the intercomparison of all the radii, with the Var. at 140 mm. as the basis of estimation. The answer to the third question is based upon careful comparison and averaging of the data, as explained below.

These tests were preceded in the case of each subject by a preliminary series in which was gained a familiarity with the general task, both as to its objective procedure and its subjective attitude. Those results were discarded. The data finally retained are divided for description into three series, namely:—

(1) *The "Norm" Series,* which follows the procedure described as the normal (page 9), and from which is derived what is

termed, accordingly, the *"Norm Type"* of visual field. (Series A and A′, Chart I and Table I.)

(2) *The "Norm Variation" Series*—involving only the changes in the position of the Variable line, and producing three series of graphs. (Series B, C, and D, Chart I and Table I.)

(3) *The Combination, or Primary, Series*—a set of data obtained by averaging together the "Norm" series and its variations, the purpose of which was to eliminate whatever peculiarities might have accrued from the influence of the factor of adjustment. This gives what may be termed the real "Normal," or the *"Primary" Field Type*. (Series E, Chart I and Table I.)

Between the first and the second parts of the above the amount of practice intervening varied with different groups of observers, as will be noted in connection with the results.

Results.

Two deflecting or secondary influences entered more or less prominently into the work of this and other sections, namely, the subjective one of *practice* and the mechanical factor of *adjustment* in the lines. Both were necessary evils which in the end may turn to good account. Each will be discussed in a later section, hence in this place only their more evident effects will be pointed out. For the matter of average variations within a series reference is made to Table VI, where typical aspects are given. Work that showed large discrepancies in this respect was discarded, and only that which is believed to be reliable is reported.

In all descriptions of results two sets of features, then, will be followed through, namely, those that are permanent or common—common to a long series of the same individual's work, common perhaps to the work of a group or groups of individuals—and dependent presumably upon a cause fundamental and primary in ocular conditions; second, those which may be transient, and hence attributable to some, evident or obscure, secondary factor.

In summarized details the results of this section are found in Chart I and Table I. The following paragraphs describe the principal facts there shown in each subject's work.

Subject Jo.

Series A—For this subject, as for all others who served regularly, graph A (Chart I) represents practically the first normal series. Its most notable feature is the fact that the lower field (M's 90′, 45′ and 135′) is in every case markedly larger than the corresponding upper parts. The left field is also larger than the corresponding right, except that the right upper diagonal, M 45, is *slightly* greater than the left upper diagonal, M 135. The right horizontal, M 0, occupies the eighth place in order of size, the lower vertical the first. The line holding the seventh place, i. e., nearest equal to the right horizontal or Var., is the opposite line, M 180.

Series A′.—The 43rd series for this observer. It is selected here because of its proximity to series B. Practice has resulted in a notable vertical lengthening of the field, with a persistence of the exaggerations of the under and left halves over their respective opposites. In other words, while practice increases all the meridional disparities, it most affects the V-H difference.

Series B.—With the Var. at the left (M 180), there is a shortening of the lines at angles *a* and *a′* and a corresponding lengthening at *c* and *c′* (the opposite field from the Var.), while the right horizontal line also becomes slightly longer than the left.

The persistent features carried over from the norm are the extreme vertical length of the field, and the overestimation of the lower half with respect to the corresponding upper portions—excepting only the case of M 135′ (angle *a′*). The overestimation of the right field in graph B is *less* than that of the left field in graph A′. This indicates that, barring the disturbing factor of mechanical adjustment (of the Var.) the left half would still appear greater than the right.

Series C.—With the vertical position of the Var. (M 90′ in this case) the field assumes more uniform proportions. A tendency to *push out* or exaggerate the field opposite the Var. is apparent again, resulting now in an enlargement of the upper field over the lower, or a reversal of the usual norm type.

The horizontal lines and the lower diagonals are *underestimated* with respect to the lower vertical, or Var. This is a feature persisting from the norm type and is common to the B series also. The underestimation of the right horizontal is greater than that of the left horizontal. This also accords with anticipation from the norm data. The right diagonals, however, agree better with the B than with the A′ series. The overestimation of both the upper and the lower vertical extents with respect to each of

the horizontal meridians remains prominent, though with a reduction of percentages. (The apparent decrease of the size of the field is due to assuming as the unit of measure the objective rather than the apparent length of M 90′, the line heretofore most greatly exaggerated in the graphs.)

Series D.—With the Var. at the upper vertical (M 90), whatever disturbing factors accompanied it are reversed in their influence from that in graph C. Accordingly, we do find overestimations reappearing in the opposite, or lower, field. The *average* (6.3 mm.) of the overestimations in the lower field in this series is slightly greater than the average (5.3 mm.) overestimation in the upper field in the preceding series C, which is so far an indication that, barring the influence of adjustment, or of angular distance, the lower field in both C and D would appear greater than the upper field. This is *true, provided* the disturbing influence of the Var. is not greater when it occurs in the upper field than when it is in the lower.

All lines in the left half of this field are greater than the corresponding ones in the right (left upper *vs.* right upper, etc.), and this again is consistent with the norm type A′, and especially with A. Again the vertical elongation of the field persists.

Series E.—The averaging of the four series last described gives a graph (E) that not only resembles the norm A, but shows more pronouncedly the upright oval form which has prevailed throughout a long series of intervening practice. It stamps finally what must be deemed an *individual peculiarity,* namely, the *tendency to overestimate the left half of the field with respect to the right and to produce a larger V-H illusion in the lower than in the upper field.*

The problem of validity, of primary and secondary causes, is crucial. Whether or not the apparent characteristics in any case may be referable to a fundamental ocular condition, or, instead, are traceable to an acquired habit or attitude, is a question for subsequent sections. Here it may be accepted, in summary for this subject, (1) that graph E is a fair average of the four series (A′, B, C, D) from which it is derived and that in it the immediate effects of adjustment have been eliminated, while (2) the results of *practice,* so evident in A′ and persistent in E, are such as to render doubtful the whole matter of primary estimations in the left field. As to the validity of the differences in the upper

and lower fields it may be said that in the intervening work between the A and A′ data the lower field was overestimated with respect to the upper in 75% of the series. As to the primary character of the vertical lengthening of the entire field there seems no controverting evidence.

Subject Pe.

Series A.—Contrary to Jo., this observer sees the upper meridians larger in every case than the corresponding lower ones. (The difference is not marked and the average variation would bring it into question were it not persistent in the later averages.) In harmony with Jo., but more pronouncedly, he makes the left field greater than the right. The overestimations of the three meridians in that field are greater than the error in either of the vertical lines. The usual V-H illusion for the left side of the field is thereby destroyed, although it is quite prominent on the right side. The upper left diagonal is first in order of size.

Series A′.—The 10th series, but typical of all later. There is noted again the large left field with increasing exaggeration on M 135; a greater difference between the upper and lower fields (favor of the former); also the considerable lengthening of the upper vertical line—which now involves an overestimation with respect to the left horizontal M.

Series B.—With the variable at the left there is marked consistency in underestimating the right horizontal line; in the small average enlargement of the upper field over the lower; in the pronounced V-H illusion for the right field; and in the slightly negative V-H illusion in the left field. The following discrepancies (indicative of a secondary influence both here and in the norm series) are notable: both M 135 and M 135′ (which are now the angles *a* and *a′*) are underestimated with respect to the Var., M 180, whereas they were formerly seen greater than this line; M 45 and M 45′ are each overestimated with respect to both horizontals in this graph; the excess of M 180 over M 0 is reduced one-third, or from 19% in the A′ series to 6.5% in this series.

Series C.—With the Var. at M 90′ there is an underestimation of all other meridians with respect to this line, with the exceptions of M 90 and M 135. The underestimations are greater upon the right side than upon the left, a fact which accords fully with the A and A′ data. The V-H illusion now prevails for both

vertical radii in comparison with either horizontal line.—Apparently the secondary factor producing these variations is here working together with what seems a constant "primary" tendency, to produce a larger overestimation in the upper field than in the lower.

Series D.—With the Var. reversed from the position of Series C the secondary factor presupposed in the above becomes opposed to the "primary" tendency as respects the vertical halves of the field, and proves the stronger. For the first time the lower half is larger than the upper, but the percentage is more than two thirds smaller than for the opposite phenomenon in series C —5.5% as against 18.2%, indicative of the *presence,* at least, of the opposing primary tendency. A further persistent norm trait is the overestimation of the left field with respect to the right in all corresponding M's. The comparison of M 135 with M 45 draws the usual lengthening of the former out of the obscurity into which it is cast by its relation to M 90.

Series E.—The peculiarities of the final ("primary" or "normal") graph are (1) that *the upper field is slightly but consistently larger than the lower,* (2) *that the left is markedly larger than the right,* and (3) *the largest overestimation lies in M 135.* These are all features so consistent with the "norm" type A as to strengthen the inference that the latter minus the effects of the adjustment (eliminated here) would coincide closely with this final graph. All the work of this subject shows unusual consistency, and with very small average variations within the series.

Subject Ta.[1]

This observer was present only for the series A and A′ and the long intervening work that led toward the remaining tests included in this group. Typical data is recorded in Table IV, and the graphs are shown, in connection with "Practice Effects," in Chart V.

Series A and A′ singularly resemble the work of Pe., with an even greater exaggeration of the typical features. There are, for example, the slight predominance of the upper field over the lower, the much greater enlargement of the left over the right, the same unusually large measurements everywhere except

[1] An unsuspected illness took Miss Taylor suddenly away from us, and left the memory of her faithful work saddened by the thought of the effort it must have cost her.

upon the diagonals of the right field, and an especially great prolongation of M 135. There is no apparent reason for this similarity. Pe. was a strong robust man, and did not wear glasses. Ta. was a slight delicate girl, and wore glasses. (Her results, however, were of the same type with or without glasses.) There was no conference whatever between the two observers concerning the work; neither knew the other's results. Each frequently remarked, when the St. was placed upon M 135, that that line looked very, very long, seemed to "stretch out infinitely."

The persistence of the norm type for this subject is seen only in the later "practice" series, A′, where practically the same general outline is preserved, though with greatly enlarged measurements.

Subject Hu.

Series A.—The early, naive work of this subject differs from either of the types described above. His is a third—unique, individual, and peculiar—type of "norm" field. The characteristics are as follows: The right field is larger than the left (contrary to both the preceding); the upper is larger than the lower (in harmony with Pe., contrary to Jo.); there is a tendency to an underestimation of M 90′ and M 135′ (as well as M 180), with respect to the Var., M 0. The V-H illusion, therefore, though prominent in the upper field, does not exist in the lower.

Series A′.—Forty-five series intervened between A and A′, the latter being the 51st for this subject. It is selected here because it is a typical norm series of the group of tests in which series B, C and D occur. Peculiar practice stages (see page 61) had marked the intervening tests since the A series, only the final aspect of which need be noted here.

Graph A′ shows everything underestimated with respect to the Var., M 0. There is a pronounced lengthening of the field horizontally, and flattening vertically, so that the V-H illusion has vanished from the upper field also, so far as the right horizontal line is concerned. It still prevails with respect to M 90 *vs.* M 180, though to a very slight degree—less than 1½%. Nevertheless, the following characteristic features of the "norm" persist: (1) the upper field is greater than the lower; (2) the right field is greater than the left.

Series B.—The graph is practically the same as that of the A′ series, with the exception that line 180 is made the basis of construction. The relative over- and underestimation continue almost in identity, with the exception of a slight shortening of

the *a* and *a′* and a corresponding lengthening of the *c* and *c′* diagonals.

Series C.—Throughout the greater part of Hu.'s work, M 90′, now the Var., held eighth place in the serial order of size. The position is maintained here, i. e., all other lines are overestimated with respect to it. The horizontal lengthening of the field persists. There is again the apparent tendency to lengthen the diagonals opposite the Var. The right field, once more, is larger than the left, the upper than the lower.

Series D.—M 90′ and M 135′, prominently short in all this subject's work, are underestimated. (Compare graphs A, A′ and B.) The entire lower field is smaller than the corresponding upper parts. Again, there is the horizontal length, greater in the right field than in the left.

Series E.—The data for this section (tabulation and graph) is simply an average to be anticipated from the series of which it is composed. The graph, carrying over the idiosyncrasies of practice from long series intervening between the A and A′ data, bears little superficial likeness to the norm A. The original prolate form has become oblate; the V-H illusion is conspicuously absent from all quarters of the field. Unanswerable evidence is this for the presence of some strong *secondary* influence, which does not bear the *usual* marks of the adjustment effects. But the contradictory features are left for a later series to resolve. (See Part III, Sections 4 and 5.) Here the essential facts are these: that in this final "average" graph the upper field is larger than the lower, the right is larger than the left, and there is a marked flattening in the lower left segment—in all of which there is harmony with the norm. Unanswerable evidence is this for the presence of some strong *primary* influence which, back of and superior to shifting secondary forces, predestines the subject to see a given objective field in his own characteristic way, which may differ in particulars from that of any other individual.

For the remaining subjects (Ba., Py., Ki., and Ca., who served only in a brief summer session) there was no long period of practice with but one position of the Var., as in the case of the preceding group. On the contrary, a B, C, and D series followed in succession after every normal series. Practice, then, may be expected to differ in its results. For three of this group of observers these results are shown in the graphs by broken lines, though here the diagonal measurements were inserted on inference, as only the four cardinal meridians were used in the

later series. The pairs of graphs will, in most cases, be discussed together.

Subject Ba.

Series A and A′.—In common with a majority of subjects this individual began her work with an overestimation of all meridians with respect to the Var. (M 0). The M 135 and M 135′ were especially pronounced in their overmeasurements; consequently, the entire left field was greater than the right. In series A the V-H illusion was larger in the lower field than in the upper; the upper and lower diagonals showed slight inconsistency, though as a whole the lower field was greater than the upper. The A′ (16th) series continues with a slight overestimation of M 180, but reverses the differences between M 90 and M 90′. (The diagonal meridians were not measured.)

Series B and B′.—The B series, except in the lengthening of the diagonals in the right field—still opposite the Var.—shows much similarity to the A′. The later (B′) series, however, marks a shortening of the vertical measurements, while M 0 seems more yielding than formerly to some extraneous influence, apparently that of adjustment.

Series C and C′.—In both instances the left field is greater than the right (in agreement with A and A′); the upper field is larger than the lower. The latter feature has in it presumably the influence of the Var. at M 90′.

Series D and D′.—Again there is elongation in the field (this time the lower) opposite the Var., with a tendency to correction with practice. The high variation between the series shown here renders uncertain any attempted distinction between the "primary" and "secondary" forces that would control the relative values of the upper and lower fields. There is in this case a nice balance between the right and left sections of the field, with the slightest difference in favor of the left.

Series E.—In the final average, *the enlargement of the left field, though small and especially so at M 180, persists. The upper vertical has become longer than the lower; the lower diagonal extents remain greater than those of the upper field. In both fields, the V-H illusion continues prominent, giving to the entire graph the general prolate form.*

There is a resemblance here to the type field of Jo., but with less consistency. The work of this subject showed a rather high average variation. She was cautious and hesitant in making her judgments—a method rarely conducive to consistent results.

Subject Py.

Series A and A'.—The graphs for this subject show a nearer approach to a circular field type than is seen in any of the preceding. The variations, in this series, between the different meridians are scarcely greater than the average variation for any single meridian alone. With practice there is a diminishing of all measurements, more on some lines than on others, and leaving a doubtful advantage over opposites in favor of the upper and left sections of the field.

Series B and B'.—The reversal of the Var. position gives a closer balance between the right and left halves and a pronounced overestimation of the lower with respect to the upper field, though the V-H illusion persists in both fields. The practice effects are ambiguous, and well within the average variation.

Series C and C'.—The first of these series shows an underestimation of the right horizontal line; the practice series shows both horizontals reduced, thereby bringing out distinctly the V-H illusion in the four relations. The upper and lower vertical lines are close to equality. All diagonals are slightly overestimated, with the exception of M45, which is underestimated.

Series D and D'.—Again with the earlier series the right horizontal is underestimated and in the practice series both right and left horizontals are reduced in their estimated lengths. The measurements in the lower field are slightly greater than in the upper. Practice shows a decrease of all the measurements and, excepting on M 0 and M 180, a nearer approach to accuracy, or equality.

Series E.—The average graph somewhat resembles that of Ba., in that it is *mildly prolate*, with *very "square" corners* which are more prominent in the lower field than in the upper, while the *upper vertical meridian is a very little shorter than the lower*. However, the small extension of the graph at M 180 preserves the resemblance to the earlier A and A' series; but the figure suggests the possibility that the successful elimination of *all* disturbing factors might reduce the outline to something near a circular form.

Subject Ki.

Series A and A'.—Here is shown an enlargement of the left and upper fields over their opposites, with a flattening at M 45'. That most of the extension on the left was due at first to the adjustment factor is apparent by the correction for this in the later, or practice, series. The field is greatest in its vertical dimension, and this is elongated most in the upper portion.

Series B and B'.—Again there is the vertical length, but a tendency (not diminished by the given practice) to push the longer measurements to the field opposite the Var.

Series C and C'.—The vertical length continues. With respect to the Var. there is an underestimation of all lines, with the exception of M 90; the V-H illusion thus continues especially prominent in the upper field, though it exists also in the lower.

Series D and D'.—True to all preceding graphs, except the earlier B, the upper vertical line remains first in the order of size; all other meridians are underestimated. The opposition of the adjustment factor is either corrected for, or it is insufficient to overcome the difference in the upper and lower parts. Practice tends toward accuracy of estimation in the vertical-horizontal relation, or a reduction of the illusion, in the upper field.

Series E.—The most harmonious figure of the series, the one approaching most nearly the perfect oval, is that of the "primary graph" of this subject. *The long diameter is the vertical, whose greatest length, however, extends into the upper field. The entire upper field is markedly, and the left slightly, greater than the corresponding opposites.* There is a small underestimation at M 45' (in opposition to Hu.'s shortening the figure at M 135'). The differences in the right and left fields are within the average variation.

Subject Ca.

For this observer the experiments of this section represent his very first work, and they were not repeated in full. There is a beautiful yielding to the influence of the adjustment factor, likewise a clear pointing to a definite *primary* given.

Series A.—All other lines are estimated longer than the Var. The greatest field length is vertical, with the greater enlargement in the upper extent. M 45' is the one exception to the enlargement of the upper and left meridians over their corresponding opposites.

Series B.—Again the Var., though opposite from its former position, becomes the shortest meridian. The vertical elongation is slightly less pronounced, indicating that in this case either the adjustment factor is slightly guarded against or else that the left horizontal is actually *seen* a bit longer than the right. The former is probable.

Series C.—The vertical lengthening is clearer in this case—two factors may be supposed to work together. The upper field

is enlarged, and the horizontal lines (and to a slight degree the lower diagonals) are underestimated. The underestimation is greater on the left side.

Series D.—Making M 90 the Var. results in an underestimation of all lines that have heretofore measured less than this line, with the exception of M 45′, which was formerly given large measurements and is here supplemented by the secondary influence of adjustment. Other lines in the lower field are pushed out with respect to the upper more than in the former series. The right field is everywhere greater than the corresponding left parts.

Series E.—This graph presents the *oval form and in the upright position. With one small exception, covered by the average variation, the upper field is consistently larger than the lower, the right greater than the left.* The left horizontal is underestimated by a small amount, which, again, the average variation in a more extended series might cover.

Summary

(I) *Varying Features.*—(1) *Practice Effects.*—For the first four subjects of the group—those having a long series of intervening tests between the A and A′—there is a marked tendency to increase the overestimations of all lines with respect to the Var. (at M 0), with the exception only of the case of Hu., where the effects were directly the opposite. For the remaining subjects, for whom the series followed in succession—A, B, C, D, A′, B′, etc.—the practice effects are less consistent, but apparently the predominant tendency is to decrease the relative overestimations of other lines with respect to the Var.

(2) *Effects of Adjustment.*—The usual effect of meridional changes in the position of the Var. is seen (graphs B, C, and D) to be a disproportioned enlargement of the field opposite to the line that varies.

(II) *Permanent Features.*—*Normal Field Types.*—In the data examined (series E, right column of table and chart) there may be pointed out three pronouncedly different types of fields under which the individual forms are conveniently grouped. They are—

(1) *The Upright Oval.*—This form, with the lower field generally larger than the upper, and the left field slightly greater than the right, is evidenced by the observers Jo., Ba., and Py.;

with the right slightly superior to the left and the upper greater than the lower, by subject Ca.

(2) *The Circular Form.*—This might be included as a subdivision under the preceding head, since the vertical still is generally the greatest diameter of the field. Yet because of diminished differences in the diametrical aspects of the figures this group is segregated. Given, then, a figure approximating a circle, but with the pole of vision several degrees "eastward" and a little to the "south" of the center, and we have the type of Pe.'s "normal" field; with the pole even farther to the "east" and farther to the "south," we have Ta.'s type. With the "corners" further rounded, but a more pointed "tip" at the vertical extremes, the pole of vision slightly to the "south" and very slightly to the "east," this group may include also the type of Ki.

(3) *The Oblate Oval.*—The third type of field manifested in the E series, though it is an oval, has its greatest diameter in the horizontal direction. This is represented by the work of a single subject, Hu., in whose case the secondary influence of practice had its most unusual effects. In the early naive work of this observer (series A) only the lower field shows this oblate flattening.

Common to groups 1 and 2, and possibly to the upper field in type 3, is the superior length of the vertical diameter with respect to the horizontal.

A regrouping of the types, not on the basis of general outline, but in accordance with the estimations of the right *vs.* the left and the upper *vs.* the lower fields, would result, with few discrepancies, as follows:

First. The left field greater than the right, the lower greater than the upper: Jo., Ba., and Py.

Second. The left field greater than the right, the upper greater than the lower: Pe., Ta., and Ki.

Third. The right field larger than the left, the upper field greater than the lower: Hu., Ca.

No case was found showing the other possible combination, namely, the right field larger than the left and the lower greater than the upper. For six subjects the left field was greater than the right; two, the opposite. For five subjects the upper field was greater than the lower; for three, the opposite was true.

The common characteristic is, finally, the evident serial place in all field types of the vertical-horizontal illusion among the many meridional disparities.

Comparisons with Data fom Earlier Investigations.—Professor Münsterberg's early assertion that the left field is seen larger than the right would be abundantly corroborated if Pe., Ta., and Jo. had been the only observers in this experiment. Scarcely a contradiction would follow from the introduction of the data of Ba., Py., or even Ki. But the work of Ca. and Hu. speaks contrarily.

The results of Professor Stevens, showing the right field greater than the left, would find a delightful correlation in the final "normal" (?) graph of Hu., were it not for the genesis of this figure.

The inference from Kundt that the right and left fields should be nicely balanced in binocular vision is uncontradicted by at least five of the subjects, in whose work there is seen much wavering with practice and a final normal graph approaching a right and left equality.

The statement from Fischer that the lower field is overestimated with respect to the upper finds a counterpart in the reiterated testimony of Jo.'s estimations. Again, the work of Ba. and Py. lends some countenance to that assertion. The contrary views of Delboeuf are amply supported by the results of Ca., Hu., Pe., Ta., and Ki., which showed the upper field magnified with respect to the lower.

The general testimony of the authorities to the existence of a vertical-horizontal disparity, i. e., of the subjective overestimation of a vertical distance with respect to an equal horizontal extent, is borne out without exception if the concession is made (in accordance with succeeding evidence) that the later work of Hu. is dominated by a secondary rather than a primary condition of space perception, and that his earlier work is the more typical of his *normal* field.

The graph of Wundt (page 13) indicates that not only M 90 and M 45 are overestimated with respect to M 0, but all intervening meridians, between 90° and 45° and between 45° and 0°,

are likewise and proportionately overestimated. We are told that similar results occur in the four quarters of the field, and that only M 180 has subjective equality with M 0. This perfect symmetry has long been supplanted, according to that author himself, by later results. The field contains, nevertheless, a hint that leaves one the less surprised to find in the data of the present experiment the near-lying standards so often overestimated with respect to the horizontal Var. (See Graphs and Tables, angles a and a'.) And since this early work of Wundt was done under entirely different conditions, as to the features both of adjustment and of practice, it must be inferred that these secondary factors cannot alone be responsible for the very "square cornered" appearance produced in certain graphs by the overestimation of the diagonal lines. (Compare the "bulge" at M 15, Wundtian graph.)

The most pronounced and uncontrovertible general truth from this series of the present investigation is that under precisely the same objective conditions and with apparently the same bodily attitude, different subjects manifest subjective types of fields differing as widely in detail as do any of the contradictory factual results reported by earlier writers. The existence of *field types,* which differ among individuals and seem relatively fixed and characteristic, is, therefore, one of the principal facts of this section. The validity of this supposed fact will be subjected to further analysis in the discussion of those supplementary tests introduced for purposes of corroboration. A second truth equally evident is that the vertical-horizontal illusion takes a serial place, usually the highest, among the common meridional disparities of the visual field.

2. *Supplementary Tests*

These tests began in the early stages of the work and at first were introduced chiefly for the purpose of making sure that no unsuspected objective factor was distorting the results. Some, however, were given for their intrinsic and subsidiary interest. None was extended enough in scope to be classed with the theoretical tests described below (Part III.), hence the entire

group of minor tests will be reported together in this place. The topics included are the following: (1) Monocular Vision; (2) Inequality of Lighting; (3) Natural Elevation of the Head in the Primary Position; (4) Astigmatism; (5) Undeveloped Mentality; (6) Comparison of Equal Lines.

(1) *Monocular Vision.*—These tests were introduced during the early "normal" series in the first quarter's work. Their occasion was the fact that at that moment the four subjects then serving (Pe., Ta., Hu., and O.) were all overestimating the left field with respect to the right and it was desirable to know if this would occur with each eye separately.

The tests were made with the two horizontal lines only (Var. M 0, and St. M 180), and were not repeated. There was a total of thirty judgments for each subject. The averaged results are shown in the following tabulation, wherein the overestimations of the left line are given in millimeters:

	Ta. mm.	Hu. mm.	Pe. mm.	O. mm.
Binocular ...	44.8±3.	.4±0.9	21.4±5.	21.0 ±5.
Right Eye...	56.6±4.	7.0±3.0	33.0±6.	29.05±5.
Left Eye.....	39.5±3.	4.0±2.0	33.5±5.	29.5 ±2.

For three subjects there was an increase of the overestimation of the left horizontal in each of the monocular series; while in the case of Ta. the results for the binocular series occupy a middle place between the estimations by the right and left eye singly.

That there should be this increase for all subjects, even for Hu., whose original normal series gave underestimations for the left horizontal, seems indicative of a secondary factor producing the change. The increase of the illusion is greater with the right eye—the one nearest the variable line, as it happens—than with the left, markedly so for Ta. and Hu. and very slightly so for Pe. and O.

In this connection it is interesting to note that in Valentine's results with monocular tests[2] the vertical-horizontal illusion was sometimes greater with either eye of the subject than with both eyes together, and again that the amount in binocular vision sometimes occupied a medial place between that yielded by the

[2] British Journal of Psychology, Vol. V, p. 8 and p. 308. 1912.

two eyes separately. On these facts this author bases an argument for a distinctive retinal condition, varying in different eyes, as the causal factor in the vertical-horizontal disparity. The finality of this argument the present writer believes is rendered doubtful by the data above recorded. (See page 93.)

(2) *Inequality of Lighting.*—The possibility that the light by some undetected difference in intensity, reflection from the walls, position of the shades, etc., might have to do with the differences of estimation in the right and left sides of the field, was next considered.

The method of testing was that of turning out one of the lights and giving the usual series with one-half of the field thus slightly obscured. The results were negative. That is, the measurements fell well within the average variation for the normal series. The final outcome of the individual field types—the great variety there shown (Sec. 1)—is also an evidence that the method of lighting exerted no controlling influence in the course of the experimental events.

(3) *Natural Elevation of the Head in Primary Position.*—Later, in the second quarter's work, there appeared what promised an interesting correlation between the attitude of the subject's head in his primary position and the type of results he gave. Six subjects had then served. Five gave evidence of seeing the upper field as greater than the lower, and the same five held their heads well erect when the eyes were in the "primary position." This position of the eyes will be recalled as one of the norm conditions of the experiment. When it was fulfilled for a new subject (Jo.) his head was thrust well back between his shoulders, so that the adjustable chair had to be lowered several inches to make him at all comfortable with the apparatus. And this was the first subject the majority of whose results indicated an overestimation of the lower field with respect to the upper.

To test the matter of the correlations it was necessary to find other subjects the shape or carriage of whose heads was similar to these characteristics of Jo. These were found, in the summer quarter, in the subjects S. and Po. They also, like Jo., were under the necessity of thrusting the head far back in order to fulfill the visual conditions of the primary position.

The results were not such as to *establish* the correlation. Po., the tilt of whose head was least marked, did overestimate the lower field with respect to the upper. But S., whose head was thrown farther back than Jo.'s, gave the opposite results. Other subjects, also, in the regular experiments occasionally gave the larger measurements in the lower field, though without the backward tilt of the head. Apparently, then, there can be no direct correlation between the attitude in the primary position and the character of the illusions. Yet this point remains to be noted: In the case of Jo. and Po. the characteristic *carriage* of the head, particularly in the listening attitude in the lecture room, was well erect, while the forward bend of the head was more characteristic of the remainder of the group, including S. The inquiry arises, may it be possible that the habitual erectness of the head (a natural assumption of the primary position?) may develop a corresponding attention attitude resulting in a livelier consciousness, on the part of such subjects, of the details of the upper field (viewed by the lower retina) than is experienced by the other group of subjects? This is a problem belonging more properly to the topic of Attention. (See Part III, Sec. 5.) That no influence from the muscular strain incident to the position entered into the results is evidenced from the series of tests involving changes in that feature. (Part III, Series 1.)

(4) *Astigmatism.*—No regular experiments were made involving this aspect of the problem. However, in the earlier part of the work when the matter of accounting for individual peculiarities first arose, the well-worn question regarding astigmatism came up. Accordingly, two subjects with opposite types of "fields" (Hu. and Ta.) consented to have their eyes tested for this defect. These tests were made by competent opticians in the laboratory of the Northwestern University Medical School.[3] The results, both as to astigmatism and in the matter of muscle balance, tested at the same time, were practically negative in each case. The former was nowhere above .5 D.[4] The very slight

[3] Thanks are due to Dr. Stella Vincent for assistance in this matter.

[4] C. W. Valentine (British Journal of Psychology, Vol. V, p. 308) has established that astigmatism not above 1.5 D. is ineffective in changing the apparent length of lines.

difference in the findings for the two subjects seemed inadequate to account for the wide divergence in their "types of visual field." These results concur with the conclusions of those who have made a special study of astigmatism in relation to the vertical-horizontal illusion, namely, that such defect is insufficient to account for the phenomena in question. (See page 74.)

(5) *Undeveloped Mentality.*—As all the regular subjects of this experiment were of a high type of mentality, it was simply a matter of curiosity that led the investigator to accept an opportunity to test one of a markedly different class. Accordingly, when a fifteen year old, slightly subnormal, girl was brought into the laboratory for the Binet-Simon tests, the writer, who assisted in that work, found it easy to introduce as a part of the day's program a series of the vertical-horizontal tests. The child, naturally obliging and willing in all she did, entered into this as a new kind of game, and one in which she was determined not to allow herself to be beaten. The primary position was not found in her case; her chin was merely rested against the rod of the headrest in such a way as to hold the head steadily in what appeared to be its normal position. Only two meridians, the vertical and horizontal, were used, and thirty judgments were obtained.

The results were not singular. There was the same type of illusion as with the other subjects (percentage of overestimation of the vertical about 7%). The chief difference lay in a much larger average variation and a greater assurance on the part of the subject of being "exactly right" in each estimation.

The results in this series are in harmony with former findings in tests made upon children[5] and inferior races[6], namely, that all are found to be subject alike to this deception of the visual sense.

(6) *Comparison of Equal Lines.*—The experiments of this

[5] W. H. Winch: "The Vertical-horizontal Illusion in School Children", British Journal of Psychology, Vol. II, p. 220.

[6] W. H. Rivers: Report of Cambridge Anthropological Expedition to Torres Strait. 1901. "Observations on the Senses of the Todas", British Journal of Psychology, Vol. I, p. 321.

group more than any of the preceding have a bearing upon the validity of the normal group of Sec. 1. The work was in three series, two of which (A and B) were given very early in the course of the investigation and the third (C) much later. The object in series A was to rule out for a time the factor of objective movement, or the adjustment of the Var. In B the aim was to discard, in addition, another previously uncontrolled feature, namely, the changing angular distance between the St. and the Var. in the successive positions of the former. The third series, C, involved the presentation of the entire "field," or the eight meridians, simultaneously.

In the first instance, series A, there was the usual comparison in the normal order of the seven remaining meridians with the right horizontal. The difference was that in this case the horizontal did not vary in length, though the subject was unaware of this fact. He was instructed that in accordance with a temporary change of plan he was to hold a card before his eyes while the "adjustments" were made and that in giving his judgments he should state his estimations of the difference in length of the two lines in terms of a millimeter stick (200 mm. in length) which he held in his hand. Which of the lines was the "Var." in these supposed adjustments was not stated to the subject. Two series were taken, the judgments alternating in pairs, in one of which the subject said, for example, "The vertical is —— mm. longer (or shorter) than the horizontal," and in the other, "The horizontal is —— mm. shorter (or longer) than the vertical." As a matter of fact, and as stated above, the two lines were in every case of exact equality. No observer seemed to suspect the ruse, though one, Jo., greatly reduced the illusions by practice during this series.

A total of twenty judgments (or a double series) was given by each subject upon each of the seven different pairs of lines.

The second series of comparisons (series B) was similarly made, except that the lines were in opposite fields, or 180° apart. Thus the upper vertical was compared with the lower, the right upper diagonal (M 45) with the left lower (M 135′), and the left upper diagonal (M 135) with the right lower (M 45′).

These three pairs, together with the right and left horizontals, which were included also in the A series, afforded a direct comparison of the right and left, and the upper and lower fields, without either the intervention of the angular distance factor or the disturbance of adjustment. The five subjects of series A and B were Hu., Ta., and Pe., whose first normal work had preceded these tests, and Jo. and Be. who began with these series.

Series C, in which the eight radii were simultaneously compared, was given at the close of the work with three of the then "practiced" subjects (Jo., Pe., and Hu.) and at the beginning of the work with a new group of observers (Ca., Ki., Ba., and Py.) who served only in the final quarter of the experiment. It was deemed possible that long experience with the adjustable line at the right might in the case of the earlier subjects have induced a habit which, carried over, influenced the results in this work, even though the factor of adjustment was now eliminated. Accordingly, the eight-equal-line test was given to the new subjects at the beginning of their work, before habit formation had commenced. It was planned also to repeat the series at the conclusion of their work (in a C′ series) and thus to test the effects of habit in their case.—The final series was carried out in the case of but one subject, Ki., and the original series failed in the case of two (Ba. and Py.) who were unable under the given conditions to control the attention shift.

The procedure was as follows: A large cardboard was prepared on which was drawn, radiating from the center, on each of the eight meridians used in the experiments, a black bar of corresponding width and length with the standards used (7 x 140 mm.). The figure corresponds with the equal radii drawn in the graphs (or with Figure 3). A perforation at the center admitted the central fixation pin of the apparatus, and the corners of the cardboard were attached to the frame by thumb tacks. The figure was covered until the subject was seated and his head adjusted. Judgments were given as promptly as possible after fixation was attained. The subject was asked to state which was the longest and which the shortest of the eight lines. Thereafter he stated the order of the intervening lengths, to the best of his

ability, with *estimations* of the differences. *Measurements* of these apparent differences were then made by sliding bits of white paper (folded through slits cut at the sides of the drawn bars) to the points where the subjects pronounced all the lines equal to the one that *seemed* the shortest. Again, as the reader understands, these lines were objectively equal *before* these adjustments.—Great accuracy cannot be claimed for this entire series. It was not repeated, and was open to the danger of wavering attention, or wavering fixation.

Results.—In Table II the full data of this experiment is given, while in Chart II are presented four of the typical resulting graphs. These are: series A for Jo., B for Hu., and C for Pe. and Ki. In Chart V the central figure in the drawing for Ta. is also from this group of tests, series A.

The principal facts evidenced are these: (1) The series A for Ta., occurring very early in her work (series number 8 and 9), is with very small deviations a close copy of the type of meridional disparities shown in the graph of her normal series A (number 5) which shortly preceded it. (The difference in the size of the two figures is marked. This may be due in part to the omission of the adjustment factor in "equal line" series, but it is doubtless occasioned largely by the change in method of obtaining the estimations. This subject protested that she was unable to give reliable statements of *how much* the lines differed in extent. By the previous method she had only to say when they were apparently equal, when not. Besides, in this case, as she described it, she was "translating" space on the distant cardboard into terms of near-at-hand space—the millimeter stick—as one might do in making a drawing or in transcribing blackboard script into pen and ink dimensions. The conviction that her estimations were inaccurate added somewhat to her feeling of confusion. Her success, then, in so uniform a "translation" is the more remarkable; and her statement of the case doubtless accounts accurately for a large share of the decrease in size not only in her own graph but in those of others of the subjects as well.)

(2) The A graph for Jo. represents his first work (excepting

very preliminary and explanatory exercises). It clearly foreshadows his first norm series A and his final, primary, graph E; there is the same typical outline.

(3) In the figure given for Hu., based upon estimations of opposite lines, in every case where a lower line is compared with an upper line, the latter is overestimated, and in every case not conflicting with these the lines at the right are overestimated with respect to those at the left. This is in perfect harmony with all of this subject's work, and the graph is seen in close resemblance to that of his original normal series A, which shortly preceded this series.—The later work, C, apparently was dominated by the attention attitude of that period; but, on the whole, there is nothing to controvert the statements based upon the subject's normal series.

(4) In the C series the graph for Pe., based upon his 34th, or *last,* series of tests, reiterates the peculiarities of all his preceding work (compare with Charts I and IV); while that shown for Ki.—representing his *first* series—is in unique harmony with all his succeeding data. (See graphs A′ and E, Chart I.)

While the graphs shown in the charts are *selected* from the possible ones of this list, it must be evident from the table (Table II) that the remaining figures, though in some cases less exact in certain details of the correlation with the normal series, bear nothing contradictory to the facts of the preceding section.—In all this work, especially that showing high consistency, the average variation was exceedingly small, less than one millimeter, for example, in the case of Pe. Such harmony of results, therefore, from methods so diverse greatly strengthens the evidence that so far at least as these subjects are concerned there exist typical peculiarities that are innate in each individual's perception of visual form.

Summary for Sec. 2.—It may be said with definiteness that neither in the equal line series nor in any other of the supplementary experiments is there found aught to invalidate the conclusions as to the type forms of the visual fields of the subjects serving in this experiment. On the contrary, in the monocular

series, in the series involving unequal lighting of the field, and especially in the equal line series, these observers verify with remarkable unanimity the truth of their results as shown in their "norm" and "primary" graphs.

3. *Place of the Vertical-Horizontal Illusion in the Meridional Disparities*

From the preceding sections, 1 and 2, is taken the following summary as to the relative position and significance of the vertical-horizontal illusion in the various field types.

There are five possible aspects of the V-H illusion in this problem. The first and broadest, perhaps the most significant, is the general relation of the vertical and horizontal dimensions of the entire field. The others are the four radial comparisons, starting from the pole of vision, namely, the upper vertical with the right and left horizontal lines and the lower, or hanging, vertical with the same right and left horizontal radii.

On the basis of the general aspect of the whole field the group types have been described. Those fields in the more prevailing form of the upright oval impress one immediately with the prominence of the V-H illusion. Those approaching the round type, or the oblate form, tend to obscure or reverse this illusion. A closer inspection, however, discloses certain other facts. By taking the final or "primary" graphs, series E, Chart I, and the norm graphs for Ta., Chart V, for a basis of inspection it is found that in only one case (Hu.'s) is the horizontal dimension greater than the vertical, while even the graphs for Pe. and Ta. (the more nearly round, or "square," type) show that the vertical line passing through the pole of vision—the M 90 + M 90′—is the *longest diameter* of the field.

A total of five graphs—those of Pe., Ta., Jo., Ki., and Ca.—shows the longest diameters in this same position, namely, the vertical meridian passing through the *pole* of the field, and this notwithstanding the variety of eccentricities at the different "corners" of the figures. On the other hand, two subjects—Ba. and Py.—show that either of the diagonals passing through the pole, or fixation point, is greater than the vertical diameter cutting the same center. But these subjects agree with the five whose longest line is the vertical in showing the *shortest* diameter of the field in the horizontal direction. This feature of the seven graphs agrees with the striking shortening of the horizontal line in the Wundtian figure (page 13). It is possible that compara-

tive *underestimation* here is as effective as *overestimation* elsewhere in the production of the total vertical-horizontal disparity.

To return from the inspection of the diameters to the comparison of the radial relationships, there is not found so nearly a uniformity of type. It is here that the individualities and eccentricities play the chief part. For those subjects—Pe., Ta., and Ki.—showing usually the left and upper fields larger than the corresponding opposites, the illusion in the upper right field (M 90 *vs.* M. 0) is the greatest of the four radial V-H disparities. For Pe. and Ta. the M 90′ *vs.* M 0 follows next, and because of the immense widening of the left field the overestimation of either the upper or lower vertical with respect to the left horizontal is very small—occupying, in fact, practically the lowest place among the radial differences of the field as a whole. For Ki., in whose case no such widening at the left exists, the M 90 *vs.* M 180 stands second, the M 90′ *vs.* M 180 last, of the four radial V-H disparities; and these are at the head of all the radial differences of his field with the one exception that what is fourth here drops to the fifth place when the entire group of radii is considered.

When the right field is greater than the left, or the lower greater than the upper, there is a corresponding shift among the various relationships of the four cardinal radii; for instance, Jo. has his largest illusion (13%) between M 90′ and M 0, while that of M 90′ and M 180 is almost as great as that of M 90 *vs.* M 0.

In the case of those subjects showing long radial extensions in the diagonal directions, the radial V-H illusion (as noted in the graphs of Pe., Ta., Ba., and Py.) may drop in some quadrants to a very minor place among the numerous disparities of the field.

Finally, to sum up the matter, the vertical-horizontal illusion, prominent in the upright and transverse diameters of practically all normal fields, is again apparent in radial comparisons, but in the latter takes among the many disparities existing between the different radii a place determined by the peculiar location of the pole of vision in each subject's typical field. That the vertical-horizontal illusion, in a linear sense, is but a typical form of radial differences is indicated, first, by its appearance in serial order among them, where all have been produced by like or similar conditions; and, second, by the fact of its being subject

with them to conditions—such as adjustment and practice—that change the amount of the different illusions. It has been shown that for a majority of the subjects it is also the most *prominent* type of radial disparity. On the other hand, it must be further evident that each diagonal-horizontal discrepancy is in turn but a varying aspect of the one controlling discrepancy between the vertical and horizontal dimensions of the entire field.

The form of the entire field, then, since the vertical-horizontal illusion is not an isolated phenomenon, becomes the crucial matter for explanation. Any theory that would account for the vertical-horizontal illusion, or *any other* radial discrepancy, must, therefore, take into account this entire subjective figure of the upright oval which represents the objective circle. It is not sufficient that one authority should explain why in his case the left side of the field appears larger than the right, when he has not extended the application to include fully the vertical-horizontal relationships, or that another should account for the numerous over-estimations of the right field in his experiments on the basis of a physiological or anatomical condition that cannot in any known way apply to the more prominent differences between horizontal and vertical diameters of the field. But the question of the underlying cause must be left until these visual disparities have been viewed in a still further relationship, namely, the foveal-peripheral.

Summary of Part I

(1) For the given objective fields, of circular form, the subjective fields normally appear of markedly varying dimensions. Among these subjective fields, as observed in this study, there exist certain pronounced group types.

(2) Within these group types there may be well marked individual variations.

(3) The vertical-horizontal illusion holds a varying place with the other radial disparities in the different types, but in the main the totality of radial or meridional differences tends to produce a vertical elongation of the entire subjective field.

(4) As the vertical meridian through the pole of vision tends to be the *longest* diameter of the field, so also the horizontal meridian, with rare exception, is the shortest diameter.

(5) Supplementary tests—involving monocular vision, inequality of lighting, elevation of the head in the "primary position," astigmatism, undeveloped mentality, and the judgment of equal lines—have but served to verify, without hint of explanation, the results of the normal series.

(6) Practice and "adjustment," features common to all series, are disturbing influences measurably affecting all types of radial disparities.

(7) Since subject to a common origin and common influence with other radical disparities, i. e., between the right and left and the upper and lower fields, the vertical-horizontal illusion must have a common explanation with the entire group of such phenomena.

PART II. FOVEAL AND PERIPHERAL MAGNITUDES OF THE MERIDIONAL DISPARITIES

The outline, or boundary, of the normal field was determined, for each subject, in Part I. It is the purpose here, in Part II, to explore central and peripheral areas lying *within* the normal field. The broad question becomes: *Are the meridional disparities discovered with the normal standard characteristic for any length of line, or is there a varying "unit" of estimation in passing from the foveal to the marginal parts of the field?* The inquiry has an explanatory as well as an exploratory interest. All other conditions, i. e., central fixation, eye muscle strain, objective details, etc., remain, so far as controllable, the same as in the normal series. Only the retinal parts affected are altered with the change in the length of lines and with their peripheral and central placement. However, all questions of an explanatory bearing are to be deferred until there have been examined, in Part III, certain of the features assumed above as constant. The immediate purpose here is, then, to extend our list of *facts* concerning the visual field. The study connects with such others as have dealt with foveal and marginal differences of estimation, i. e., with direct and indirect vision.

Three authorities, Chodin,[1] Fischer,[2] and Münsterberg,[3] have

[1] Von Graefe's Archiv für Ophthalmologie, B. 23, S. 92. (1877).

[2] Von Graefe's Archiv für Ophthalmologie, B. 37, 1, S. 97-102; 3, S. 55. (1891).

[3] Beiträge zur Experimentellen Psychologie, H. 1, S. 125. (1889).

investigated the applicability of the Weber-Fechner law[4] to visual measurements, but with differing conclusions. Chodin finds that with lines in the V-H relationship, "the accuracy of estimation decreases gradually with the increase of distance, i. e., with the length of St., except that with large distances it increases again." This is to say that the illusion is larger with the longer extents up to a certain limit, beyond which the lengthening of the standards causes the illusion to diminish. Münsterberg, whose work showed much irregularity of result, held that the Weber-Fechner law does, nevertheless, prevail in foveal-marginal disparities—or those existing between direct and indirect vision. These he reduces to a matter of muscle strain, to the *intensities* of which—as to the intensities of light and other sensations—the given law applies. Fischer, again, holds that a modified form of the Weber-Fechner law prevails in meridional estimations; that is, he accounts for the V-H disparities on the basis of a supposition that the eye has different "Maasstäbe," or measuring units, for the different meridians of the field. Upon the supposition that these units prevail for any length of line, the following formula is given for the relative objective lengths that appear equal in the four principal meridians of the binocular field:

Lower	:	Upper	:	Left	:	Right
100		103.22		114.49		115.44

This means, of course, that the lower vertical is overestimated with respect to the upper vertical, that the left horizontal line is overestimated—to a less degree—with respect to the right horizontal line, and that both the lower and upper verticals are greatly overestimated (approximately 11% to 15%) with respect to the right and left horizontal lines. This author also found that in halving one of the four radii the outer half was underestimated with respect to the inner.

Professor Steven's results[5] are contrary to the last mentioned item. That is, marginal parts were shown to be overestimated

[4] This law would imply here that with uniform stages in increasing the stimulus there would follow a uniform increase (i.e., an equivalent percentage) in the amount of the illusion.

[5] Psychological Review, Vol. XV, p. 69.

with respect to central areas. This would point, not to a Weber-Fechner uniformity, but rather to an increasing, or at least a varying, percentage from the focal to the marginal end of the given lines.

In the present study, connecting itself, as stated, with the "normal" problem of Part I, three questions will be considered: (1) Will the same individualities found in the normal type forms prevail with shortened standards, other conditions remaining as before? (2) Will such shortened standards medially and peripherally placed (i. e., with the central junction of the two lines broken by increasing stages) yield again the typical meridional illusions? (3) Finally, are all parts of a given pair of lines equally responsible for the disparity in their subjective estimations, or is the normal illusion produced more by one part than another of the given lines?[6] What, in other words, is the percent of the illusion in the successive segments studied? Is it uniform or varying?

This is an ambitious field of inquiry within itself, but unfortunately the work here had to be abridged because of the necessity for an abrupt closing of the experiments before their conclusion. However, such results as were obtained (nearly 10,000 separate judgments) are deemed worth recording.

The subjects serving were those of the summer quarter (1912), namely, Ba., Py., Ki., and Ca., whose work varied markedly in type from that of the remaining subjects who had served from two to four quarters in the experiment. There were two series, in accordance with the first two questions named above, and from these the data for the third topic also was obtained.

1. *Central and Medial Field Types*

Briefly stated, the problem of this section is to determine the effect of variation in length of standard, the point of junction of the two lines continuing as the center of fixation. The three

[6] Note: In this study the foveal parts were compared with foveal, and peripheral with peripheral, on the same meridians used in the preceding sections. There was no direct comparison between foveal parts and peripheral parts, as in the work of Professor Stevens.

standards employed were the following: St. 1 (the normal of the preceding series), 140 mm.; St. 2, 85 mm.; St. 3, 30 mm. The visual angle and retinal extent covered by the three were respectively as follows: 6° 12′ and 1.73 mm.; 3° 46′ and 1.95 mm.; 1° 20′ and .37 mm. A preliminary series had involved the use of standards of three different lengths (260, 210, and 160 mm.), but there the purpose was to find a convenient norm for length. This study did not involve the extreme periphery of the field, but was limited almost entirely to the region of clear direct vision. The retinal image of St. 1 extended little beyond the macula lutea in its horizontal dimension. (See page 83.) St. 2 outlined a field a large section of which was wholly within the macular area, while St. 3 described a circle which in retinal space covered only the fovea and its immediate boundaries.

The procedure, with the exception of the amount of work covered, was the same as in former series. Three subjects finished one complete series (A), with all the meridians, with each standard. Thereafter, because of limited time, they were restricted to four meridians, or radii, namely, the upper and lower vertical and the right and left horizontal lines. For one subject (Ca.) all tests with Sts. 2 and 3 were limited to two meridians, the upper vertical and right horizontal, on which the positions of the St. and Var. alternated in successive series.

Results.—(See Chart III and Table III, Part 1.) The absolute and relative errors[7] for series A and E are given in the table; the intervening series, B, C, and D, are omitted from the tables, as they offer nothing new. The graphs for this section (Chart III) are superposed, the smaller lying within the larger. The lengths of the shorter standards are indicated by heavy dots. The over- and underestimations are represented, as formerly, on a scale five times too large in proportion to the length of the standards. The entire field, or largest graph, may be thought of as the "norm" field; the middle sized graph, that of St. 2, as

[7] An "absolute" error is, as formerly, the amount in millimeters the Var. departed from equality with the St. when the lines appeared equal subjectively; the "relative" error is the *percent* of the illusion, with the length of the standard as the base.

the "medial" field; and the small central section, built around St. 3, as the "central" field. These are indicated on the graphs and in the table by the letters *N, M,* and *C.*

Series A.—Of the three graphs presented two (for subjects Ba. and Ki.) indicate a fair degree of harmony in the field outlines—central, medial, and norm. With further practice, or with the average of a longer series, the likeness of outline doubtless would have increased rather than diminished. Practice effects previously pointed out indicate this possibility. The third graph (that of Py.) shows in the norm field a beautiful smoothness of outline, but with the decrease of the standard lengths there begins a series of reversals of the V-H illusion which breaks the form of the graph. The tabular data for this subject shows St. 3 underestimated everywhere except upon two meridians of the lower field. St. 2 is underestimated upon two meridians, notably upon M 90. The data for Ca., whose graph is not given, shows in his one series (see table) the prevailing tendency toward smaller absolute disparities with the decreasing standard lengths, but the table of relative values shows, nevertheless, the errors in the smaller fields are disproportionately large. This is doubtless due to this subject's habit of overcorrection for the influence of adjustment.

Series E.—A superficial glance at the graph of series E (the average of A, B, C, and D, for the four cardinal meridians only) will indicate that some force stronger than the influence of adjustment is controlling the type forms, for subjects are seen to repeat in significant details characteristics of their A series.

But individuality of type presents here a new feature. Not only do the types differ from individual to individual, but the same subject (e. g., Py.) may show markedly different outlines, or meridional relationships, in the marginal, medial, and central fields.

2. *Peripheral Comparisons*

Reversing the procedure of Sec. 1, that is, shortening (or separating) the lines by increasing stages at the central ends, there is obtained a supplementary set of data, of similar import to the preceding, but in which peripheral rather than central areas are examined.

The device employed to attain this end was that of placing circular discs, of pale yellow paper, over the center of the field, thus cutting off the central ends of the lines. (See Fig. 2, C.) The slight color contrast with the surrounding field was intended to emphasize the circular form, or the fact of the equal distance of the given lines from the fixation point. The discs used (see Fig. 6) were of the following radial dimensions: D. 1, 7.5 mm. (barely breaking the connection of the St. and Var. lines); D. 2, 30 mm.; and D.3, 85 mm. Used successively with St. 1 (140 mm.), there are obtained three figures in addition to the normal field. (See Chart III, Part 2.)

Discs 1 and 2 were also used with St. 2 (85 mm.), and so within the "medial" field there were *three* figures for comparison, including the normal of this series. With St. 3 (30 mm.), two figures were obtained, as the first, or smallest, disc could again be used, thus marking off the peripheral boundary of the "central" field also.

Results.—Table III, Part 2, gives in percent the amount of the over- or underestimation of the upper and lower vertical and the left horizontal lines, M 0 being the basis of comparison. This is an E′ series; i .e., the average of series A, B, C, and D is taken *after* a previous average has been made of all possible values in the case of each segment, obtained by the intercomparison of data. (See explanation, Sec. 3). The graphs of Chart III, Part 2, are illustrative of the type figures produced by this series, and are based upon the absolute overestimations of the A series, subject Ba.

Reading the tabulation from left to right the following facts become apparent:

(1) *Upper Field, or M 90.*—There is a comparatively uniform tendency to increase the error of overestimation as the distance between the lines becomes greater, i. e., with the use of the larger discs. The principal exceptions are in the outer rims, where the lines are very short; e. g., St. 1-D.3 for Ba. and Ki.; in the outer part of the medial and central fields (St. 2-D.2 and St. 3-D.1) for Py.; and in Ca.'s work, particularly with the shorter standards. The reversal of the illusion occurs once for Py.,

twice for Ki., with St. 3. For the three subjects whose data was most complete there are but five exceptions (out of twenty-seven cases) to the general tendency to increase the overestimations as the central space between the lines becomes greater—or as the lines themselves are more and more reduced to marginal positions.

(2) *Lower Field—M 90′*.—Here there is less of uniformity, but the errors either of over- or underestimation are in a very large percent of the cases decidedly smaller than corresponding ones in the upper field. (This *may* mean a greater ease of judgment in comparing these lines in the lower vertical extent with corresponding parts of the right horizontal than is found in similar comparisons for the upper vertical line; or it *may* mean that eye movement—shift of attention—toward the lower field is more difficult to check.—This, however, is but a word in advance of the final effort at interpretation.)

(3) *Left vs. Right Horizontal*.—The differences between the right and left horizontal lines are yet smaller—with but rare exceptions, usually with the very short lines—than any of the errors previously pointed out. It was found in the normal series for these three subjects that with practice the right and left sides of the field approached a nicer balance. That the same would have followed here with practice is at least suggested by the character of the data. That the general tendency to increase the overestimation or decrease the underestimation with each remove from the center of the field persists here is also to be noted. There is but one possible exception to this last point, i. e., St. 2-D.3, for Ki.—a case easily covered by the average variation.

This section is chiefly significant as a supplement to Sec. 1. If in this series the percent of the illusion is found to increase as the stimulus is shortened at the center, or what is the same thing, is thrust toward the margin, while in Sec. 1 the percent was seen to decrease as the marginal ends were removed, and the lines were thus rendered more and more of a central character—then there is the double evidence that more of the total meridional disparity in a given case must be credited to the marginal than to the foveal regions of the normal field. The closer investigation of this point falls to the next section.

3. *Percentage of the Illusions in Foveal, Medial and Peripheral Segments*

The third inquiry of Part II is based upon the combined data of the preceding sections, by which it becomes possible to examine the zonal segments into which the field is now divided. Figure 6 makes clear what these zonal areas are.

The following are the questions: (a) Is the percent of the given meridional disparities uniform or variant in the successive zones; and, finally, (b) What part does each segment contribute to the total illusion of the normal standard, i. e., to those disparities of the "normal field" recorded in Table I?

Now since the normal standard is 140 mm. in length it might seem desirable to have had the four segments here of equal value, that is, 35 mm. in extent. But it was found in selecting the shorter standards of Sec. 1 that a difference of less than 55 mm. in the stimulus gave results differing from the normal less than the variations in the successive normal series. Hence, 85 mm. and 30 mm. were chosen for the shorter standard lengths, and discs 3 and 2 were made to correspond respectively with these. Thus the two outer borders become 55 mm. each, while the remaining two are determined by the radius of the smallest disc (7.5 mm.) and the difference between that and the length of St. 3, which difference is 22.5 mm. The four segments, then, counting from the center outward, are as follows: S. 1, 7.5 mm.; S. 2, 22.5 mm.; S. 3, 55 mm.; S. 4, 55 mm. See figure 6.

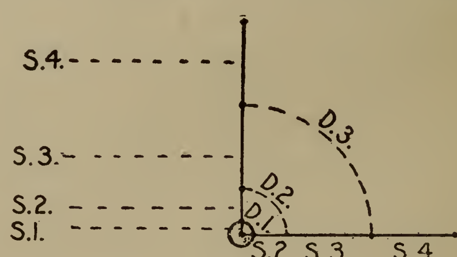


Fig. 6.

Now the value of S. 1 is nowhere directly determined, since a standard of so short a length was impracticable. It is obtained,

therefore, by the indirect method of comparison. By subtracting the value of the illusion of St. 3 (30 mm.) when Disc 1 is used from that of the same standard when no disc was used an approximation of the illusion in S. 1 is obtained. In the same way St. 2 and St. 1, by a comparison of their values with and without the small disc, afford each an additional estimate of S. 1. The three values of this segment are then averaged for the final approximate value of the tables. The value of S. 2 is obtained directly by the use of the 7.5 mm. disc (D. 1) with the 30 mm. St. (St. 3); of S. 3 by the 30 mm. disc (D. 2) with the 85 mm. St. (St. 2); and of S. 4 by the 85 mm. disc (D. 3) with the normal or 140 mm. St. (St. 1). In all of the A series, Table III, Parts 2 and 3, the values are thus obtained for segments 2, 3, and 4, and that for S. 1 by the first method named above, i. e., by the values of St. 3. For the A′ series all possible values for *each* segment are averaged; for it is quite as feasible to obtain by the indirect method of comparison additional values for the remaining segments as it is for S. 1.

The values for each segment in series B, C, and D, were obtained as in series A. Likewise these were subjected to intercomparisons, as for the A′, thus affording the B′, C′, and D′ series. By averaging A, B, C, and D, the data for the usual E series was obtained; and by averaging the A′, B′, C′, and D′, there was evolved the final series E′. In the above calculations the absolute values of the errors (i. e., in mm.) were used. The final proceeding in each series was to obtain the *relative* values, by reducing the data to the percent form—which is given in the tables. The method, of course, was that of dividing the amount of the illusion in each instance by the length of the standard concerned. This gives the data for Part 3, *a*, of the table. By dividing these same absolute values by 140 mm. there is found the relative amount (or percent) contributed by each segment to the total illusion of the normal standard. This is shown in Part 3, *b*, of the table.

Results.—(a) *Segmental Disparities.*—The indications of sections 1 and 2 as to the relative amounts of the V-H illusion in the different parts of the field are borne out in the combined data. Following are the more detailed facts.

Series A and A′. M 90.—The table (Part 3, a) shows that for three subjects the illusion of the upper vertical tends to diminish in percentage or be reversed (i. e., become negative) in an increasing percent, in passing from the margin inward—reading the tabulation from right to left. Reading from left to right it is clear that the positive illusion increases with each outward remove, i. e., from the foveal area to the medial and marginal segments. Ca.'s data are directly the opposite to this in series A, but conform in series A′.

M 90′.—In the lower field the regularity is less perfect. The negative illusions extend for some subjects further from the center, or appear irregularly. But it is still evident that the largest illusions occur in the more marginal segments.

M 180.—In the values of M 180 (compared again with M 0) the positive illusions, and usually the larger ones, fall once more in the outer segment.

Series E′.—When the factor of adjustment has been eliminated (and that after averaging the data in each series) it is still evident that for some subjects the illusion of the upper vertical is enlarged in the outer segments, i. e., S. 3 and S. 4, though three subjects show a higher percentage in S. 3 than in S. 4. This may be due to the fact that fewer values of S. 4 were obtainable for the averaging than in the case of S. 3. But it is necessary also to recognize that the direct value of S. 4 (St. 1—D. 3) was frequently negative.

(b) *Relative Contribution of the Segments to the Total Illusion of the Normal Standard.*—Peculiarly interesting facts arise from reducing the illusions of the separate segments to a percent of the normal standard. In this way it is to be determined whether or not the sum of the illusions in the parts will approximate the total normal illusion, or that observed when St. 1 is used alone.

Part 3, b, of the table gives both in millimeters and in percent (the latter on the basis of St. 1) the illusion of the norm or St. 1, the illusion of each of the segments separately, and finally the *sum* of the illusions in the separate segments.

The following are the facts indicated: (1) In the E′ series (shown in M 90 for all subjects) the difference between the sum

of the parts and the illusion of the whole is in every case in the neighborhood of 1 or 2 mm. and 1 or 2 percent—or what would be a small average variation in a normal series. As an example, in the data for subject Py. the sum of the illusions of the parts equals 6.02 mm. or 4.3%; the illusion of St. 1 is 5.06 mm. or 3.61%; the difference is .92 mm. or .69%.

The additional data given for Ba. in the table, indicates, in series E′, similar facts in the lower field, and even in the left horizontal, though there the dfference is greater, while in series A′ and A the correspondence (M 90) is extremely close. The difference in the latter series is less than one-tenth of one percent. Corresponding data for the other subjects, not printed in the tables, indicates a similar harmony.

In the majority of cases it may be noted that it is the sum of the illusions in the parts, rather than the total illusion of St. 1, that is slightly in excess over the other. *Possibly* this may indicate for these subjects a slightly greater difficulty in judging small lines.

It is now possible to inquire intelligently as to the contribution of each successive segment to the illusion of the whole line. Reading the data from left to right (S. 1 to S. 4) it is seen that the V-H illusion, sometimes negative in S. 1 or S. 2, generally increases in positive amount at successive removes toward the periphery. There are two exceptions—the slight one in the work of Ki. and the more marked in that of Ba.—wherein the illusion is less in S. 4 than in S. 3. This now occurs for Ba. only in the E′ series; the preceding series are freed from the exception, which was more common in section *a* of the table. In the main, then, the statement is fairly accurate that, as our field is divided, it is the outermost segments that contribute most largely to the meridional disparities of the entire field, while a questionable negative factor is discoverable in the central parts.

Comparison with the Data of Earlier Writers.—The phenomena of Part II of this experiment, including the "exceptions," are more in harmony with the finding of Chodin[8] than with those

[8] Whereas Chodin found the amount of the V-H illusion to increase with increase of standard length, up to a certain limit, then diminish again, this

of Fischer and Münsterberg. If there exists a "unit of estimation," varying from meridian to meridian, as Fischer claims, then it is apparent that it also varies from fovea to periphery along the same meridian. This would account for, or at least restate, the results of Prof. Stevens. The further supposition, that the rate of variation from the fovea peripheral-wards increases more rapidly upon some radii than upon others, unites the phenomena of this experiment with those shown in Prof. Stevens' work, while the individual variations above noted would rest upon a supposed irregularity in the variations of the "unit" in the different sectional parts of the radii. Certainly the phenomena are related with sufficient closeness to indicate that whatever is assumed as an explanation of foveal and peripheral differences of magnitude should be sufficiently broad to account also for fundamental facts of the vertical-horizontal illusion and kindred disparities.

Summary of Part II

(1) In the smaller, "central" and "medial," field outlines, described within the "normal" field, there exist again the V-H illusion and certain individualities of field types.

(2) In the marginal and medial *zones* of the normal field the typical disparities in meridional estimations reappear. That is, in lines shortened, or separated, at their central parts the illusions persist.

(3) The percent of the V-H illusion—sometimes negative in the foveal area—tends to increase in passing from the central to the peripheral parts of the field, at least to a certain limit, beyond which it *may* decrease again. That is, the "unit of estimation" was found to vary in successive parts of the different radii examined.

(4) The total of the illusions of the separate segments approximates closely the amount of the illusion of the norm standard taken as a whole. To this total illusion the marginal parts apparently are the largest contributors, while the central extents may tend to negative or reduce the total effect.

(5) From these facts it seems probable that the vertical-horizontal illusion connects itself with a series not only of meridional

experiment demonstrates that other disparities in estimation of meridional lines (*e.g.*, M 180 *vs.* M O) may be subject to a very similar variation. Doubtless differences of attention attitude are involved.

but also of foveal and peripheral disparities in subjective estimations of magnitude.

PART III. DETERMINING CONDITIONS: CONTROL TESTS

Certain supplementary tests earlier in the work examined minor conditions which were thought of possible influence in the outcome. These were not found effective. (See Part I, Sec. 2.) It is purposed here to examine in detail, and in their theoretical bearings, the following factors: (1) Ocular Position; (2) Bodily Position; (3) Objective Contour; (4) Practice Effects; and (5) Attention Attitude.

1. *The Effects of Ocular Position*[1]

The recognized inverse correlation of the relative strength of the ocular muscles with the directions of over- and underestimations in the visual field has rendered the Wundtian theory a popular one.[2] But since other physical features (e. g., contrast with the shape of the objective field) correlate quite as well, theoretically, with the facts of vision, it is possibly unwise to accept such a chance correlation as conclusive evidence of a cause and effect relationship. The apparent inaccessibility of the muscles themselves to experimental attack long served to keep the problem out of the laboratories. A study was made, however, some years since by Rivers and Hicks.[3] Their device was that of parallel series of momentary and prolonged exposures. The former, less than 1/50 second, were so brief, supposedly, as to exclude any actual eye movements, at least such as would aid in the estimations. Though they admit that Wundt's auxiliary doctrine of the *tendency* to movement as a possible basis of judgment has not been answered, their data is so clearly negative as to the value of the actually performed eye movements, that these authors and their followers[4] conclude the whole theory is groundless. The present experiment deals with the problem from an opposite line of approach. Instead of attempting to render mus-

[1] This series was at the especial suggestion of Professor Carr, and was the starting point of this investigation.

[2] Wundt: Outlines of Psychology, pg. 135.

[3] British Journal of Psychology, Vol. II, p. 234.

[4] Valentine: British Journal of Psychology, Vol. V, p. 8.

cular effort impossible, the purpose is to increase the strain, to induce effort. The thought is that whatever augments the *cause* of the illusion ought to increase the *amount* of the illusion, and the reverse also should be true. If in a position of intense strain of the superior or inferior recti muscles the vertical-horizontal illusion is found to increase, while with induced strain in the interior and exterior recti there follows a reduction of the illusion, here would be a correlation well worth recording in substantiation of the theory. If such results do not follow, one may at least look elsewhere for effective agents.

The specific procedure was as follows: A practice series of the normal order had been given until the subjects (Jo., Hu., Ta., Pe., and Be.) were all fairly well adapted to the experiment, that is, the variations between the "in" and "out" series, for example, were reduced appreciably. Other conditions remaining normal, there were introduced four successive changes in ocular position, effected by changes in the angular relation of the mouth bit rest to the objective field. Changes of 15° were at first tried, but proved so little disturbing in any way that 30° alterations were adopted. The four positions were as follows: (1) The head tilted backward, eyes looking downward across the lower face to the usual fixation point; (2) the head bent forward, eyes looking out across the brows; (3) head turned to the right, eyes looking left; and (4) head turned to the left, eyes looking right. Rest and normal series intervened between these successive changes.

Not only strain of the ocular muscles entered into this experiment, but the disturbance of the bodily attitude as well. The new positions of the eyes involved, further, a change of contour —or a modification of the natural oval of the visual field (in its entirety, rather than in its restricted sense of this paper). To illustrate, when the eyes were turned far to the left, the oval was shortened upon the right; turned to the right, there followed the reverse shortening. The adjustments up and down narrowed the oval. In addition, the features of the face—brow, nose, cheeks, etc.—obtruded unnaturally into the field with the different tests.

Three things are thus involved in this series: (1) the eye strain and eye movement theory; (2) the secondary factor of bodily discomfort and disturbance, together with intensified attention strain; (3) the Külpe contour theory. The greatest significance attaches to the first of these.

Results.—Unquestionably all subjects were disturbed, rendered uncomfortable, by the strain of bodily as well as of eye position, and by the consequent feeling of effort in attention. Table IV is a full record of the numerical results. The following are the principal facts: (1) The general form of the "fields" remains, with slight exception, noted below, as in the normal series. (2) The average variation, both between series and within the single series, is much increased. Such irregularities as Pe.'s occasional larger estimation in the lower field than in the upper, occurred also in a small precent of his normal series. (3) A general increase in the amount of the disparities is noted for all subjects, excepting Hu. In the case of the latter the reverse is true. Not only do the illusions diminish but they tend to be reversed in character, i. e., to become negative. (4) The features named under "3" persist and become more marked in the succeeding "normal" series. They are shown in a later section (Sec. 4) to be a part with the typical *practice effects* for each subject concerned.

The conclusion is evident, namely, that there follows from this test no type of result that is constant to or dependent upon eye position and muscle strain. It is further evident that the involved change in contour of the visual field is alike ineffectual in modifying the type of illusions shown. This, however, will be deferred to another section for discussion. These findings coincide with the negative results of Rivers's tests of muscle strain effect by its elimination. Whatever correlations may exist, then, between muscular asymmetries and the peculiar meridional disparities of spacial judgments seem no nearer than before to a substantial proof of a cause and effect relationship. Indeed if the elimination of the muscular effort and also its increase to the point of extreme intensity *both* fail to affect the amount of the illusion in the way demanded by the theory, there seems

a fairly positive proof that no such causal or automatic relation exists between the muscular movements and the visual spacial apprehensions.

2. *The Effect of Bodily Position*[5]

This series of tests, following shortly after that of the preceding section, involved a complete change of the subject's relationship with the whole objective world exclusive of the particular *visual field* with which the test is concerned. That changed with him. A couch and a pillow were added to the equipment and the principal apparatus lowered to the level of the subject's eyes; fixation was required as before with the head in as comfortable and natural a position as possible but without attempt to use the mouth bit rest. Four positions of the body were involved: the normal, the reclining to the right, reclining to the left, and the inverted head. In each of the attitudes the subjects found the variable line in the right field of vision—its accustomed place up to this stage of their work; "up" was in the direction of the subject's head, "down" in the direction of his feet. It follows, then, that with the reclining to the left the variable is upon the upper vertical with respect to the earth, the reclining to the right gives the variable the position of the lower vertical with relation to the earth, and in the inverted head position the variable is upon what is normally the left horizontal line. In all these changes, however, the variable is still the "M o" and the other meridians are designated accordingly, i. e., with respect to the subject rather than to the external world. There is, then, no change involved in the bodily or ocular relation of the subject

[5] The original suggestion for this section comes from Professor Angell. Mr. A. F. Buck made a brief investigation of the same problem in this laboratory some years ago. His subjects were placed in two positions, sitting and recumbent. Cords were used for the adjustments; standard lengths 20 cm.; distances from the eye, 60 cm. and 120 cm. Retinal arcs thus involved were wider, marginally, than in the present experiment. Since he does not find the illusion translated with bodily changes in position, and since his tests with eye movement give nearly identical results with those without eye movement, Mr. Buck concludes that "it appears improbable that the eye muscles at present play a dominant part in producing the illusion." Chicago Studies, Vol. I, No. 2, p. 7.

to the field as presented by the apparatus. With the same fixation at the center, and normal straightforward direction of the eyes, the retinal field affected is in all points practically the same as in all preceding normal series. The balance of the ocular muscles is the same, except for whatever change gravity or the new bodily position may introduce in the balance. Indeed, save for the inverted head test, the strain upon the eyes and the entire body was the very opposite from that existing in the preceding series. There the attitude was tense and very taxing; here in the reclining position everything was conducive to relaxation, even to somnolence. For these busy students the recumbent attitude was associated only with sleep. This tendency to drowsiness was the chief *disturbance* of the attitude, and the only *effort* involved was at wakefulness and at adjustment to the changed situation.

The questions involved are: Will a state of relaxation show results different from those of the normal or tense state of muscle balance? Will the change of external conditions be effective upon the relative lengths of lines in upper, lower, right and left fields? Will these meridional disparities remain fixed with respect to the retina or with respect to the earth? (The latter would connect with the whole question of openness of sky and nearness of earth in the origin of the vertical-horizontal illusion.) Further, will *mental* attitude toward the changed relations be effective? Lipps has said that, for instance, in an equilateral triangle the mental conception of any one of the lines as the horizontal base results in its underestimation and the corresponding overestimation of the other two lines. He further asserts that gravity is one of the influences entering into the "expansion and contraction and erection" of lines, and hence into their apparent length. In this series the effect of gravity in the V-H relationship is reversed from the normal.

The subjects in this series were Jo., Pe., and Hu. The results in detail (see Table V and Chart IV) are as follow.

Results

Pe.—With each change of the position of the body the characteristic "norm" graph reappears, except that the lower vertical

is measured slightly larger than the upper vertical and with the inversion of the head the enlargement toward the left and upper left field is made more pronounced by slight underestimation in the 45° meridians. The measurements in the right field, in other words, are reduced with the inverted position of the head. This does not appear significant in view of the general shape of the graph.

Jo.—The most uniform results were produced by this subject. The first and second series gave graphs falling well within the average variation of the normal series, while the inverted position of the head gave what might be accepted as a beautiful reproduction of the "primary" graph (Chart I, E). One discrepancy occurs in the first graph of this series (left reclining position). There is a slight bulge on the left side of the figure, or that part corresponding to the lower field as to the earth. This hints of a carrying over of a "mental attitude," since in the normal field of this subject the lower part was overestimated. But if there *were* such a tendency it was insufficient to distort the entire field and there was a quick readjustment to it, since it nowhere reappears.

Hu.—In the case of this subject nothing occurs with these variations except the increasing amount of the underestimations with respect to the Var., and this has been noted several times in connection with other series. The graphs, here drawn on the basis of M 180, are in close conformity with those for the same subject in Chart I.

The marked feature of the above results is the recurring, with all the changes of bodily position, of the type graph characteristic of the earlier and normal work of each subject.

In the matter of bodily position, then, as of the ocular, it seems safe to assume, in view of the negative results, that unless some chance attention or judgmental factor entered occasionally from these sources, the factor of eye or bodily position was not a disturbance in the general course of the experimental work, and that the results of the experiments in general must be attributed to influences more persistent and fundamental.

3. *The Effects of Objective Contour*

To Oppel[6] and Külpe[7] is due the suggestion of the position of the eyes in the face and contrast with the resulting oblate form of the entire visual field as the possible occasion of the vertical-horizontal illusion. Valentine[8] put the matter to experimental proof. He shortened the oval of the field by monocular tests, and found—the illusion frequently *greater* than in his corresponding binocular series.

In the procedure of the present experiment the variations of contour were effected by cutting the desired figures in gray cardboard and placing these in turn over the normal background, which, it will be recalled, was a circle of gray surrounding a white field. The forms employed were four in number but, by changes in position, afforded six varieties of fields in addition to the normal. These were: the oval, placed (1) horizontally and (2) vertically; the semicircle, (3) upper and (4) lower fields; the semi-oval, (5) upper and (6) lower fields. The radii of the semicircle coincided with those of the regular circle of the normal field, as did also the longest radii of the oval figures. The shortest diameter of the latter figure was 380 mm., or the least distance practicable with the length of standards in use. To insure the subjects' consciousness of the change of outline, bits of colored paper—red, yellow, or combinations of yellow and red, blue and yellow, etc.—were attached to the points of the ovals farthest removed from the central fixation point, or else opposite the ends of the lines to be judged. The subjects serving were Hu., Ta., Jo., and Ca. The usual double series of tests was carried through, for three of the subjects, and typical series were given to the fourth (Ca.) The mouth bit head rest was again used and the "primary position" maintained.

Results.—No tabulation of the results of this series is given in this report. For any subject the normal series just preceding this group of tests may be taken as typical of the results here. The variations are similar in kind with those of the practice

[6] See references, p. 1.
[7] Outlines of Psychology, p. 366.
[8] British Journal of Psychology, Vol. V, p. 8.

series of the normal order. Subjects continue to increase the size of their measurements, but this feature is independent of the presence of the unusual in contour.[9]

Such findings accord with the negative results of Valentine. In so far as this experiment bears a small weight of evidence neither the horizontal arrangement of the eyes in the face nor the sky-earth relationship—nor the "aesthetic" effect of gravity—can be considered effective influences in determining the meridional disparities of the visual field.

There is left an admitted possibility, not touched here, that habits long established, resulting from such external causes, may not be amenable to momentary laboratory control. But the contour theory itself is equally without any positive evidence in its favor.

4. *The Effects of Practice*

This topic bears two aspects, namely, the general effects of continued practice (present from the beginning in each subject's work), and a special series here introduced for the purpose of a separate study of this feature.

General Practice Effects.—The cumulative effect of practice in succeeding series (many of which fell chronologically between the A and A′ norm series) is shown in the graphs of Chart I, and the matter is further supplemented by Chart V. (See also Table VI.)

Briefly, these effects are as follows: (1) With those subjects for whom the position of the variable line was altered in successive series the tendency is more to *reduce* than to increase the illusions with practice, (see graphs of Ba., Py., and Ki., Chart I), and for these subjects there appears finally (normal

[9] The changes in contour, it is true, were accepted by these subjects with a determination not to be diverted from the central object in the field. The paper "flags" especially were met with sniffs. All said, in effect, with Ta., "We are not kindergartners to be influenced in our judgments by a bit of bright paper; besides it is not so bright that far out in the margin." But the very comments assured the experimenter that the paper was noticed and hence was serving its purpose, namely, that of making the subjects conscious of the periphery while attending to the center of the field.

graph E, Chart I) an almost equal balance between the right and left horizontal lines, with respect to which all other meridians of the field incline to overestimations—and overestimations of such a type as to produce ultimately a fairly harmonious vertical lengthening of the entire field. (2) With those subjects, on the other hand, for whom the variation in the position of the adjustable line was one of the last series given, the tendency in the main was toward increasing overestimations of all other meridians with respect to M 0—the Var. (See Jo. and Pe., Chart I; also Ta., Chart V.) The personal peculiarities of this latter group of subjects (pointed out in Part I) not only continued but increased in prominence in successive series. The only proof, then, of any valid primary difference between the individuals lay in the fact that these peculiar traits appeared in series wherein either practice or adjustment, or both, were non-effective. (See E graphs, Chart I; also "Equal line" graphs, Chart II; and the central outline for Ta. in Chart V.) (3) Subject Hu. is in a class to himself as to results, but is in the second group, above, as to method. (See Chart V.) For his first dozen series he continued to give results of a type with those of graph A (Charts I and V). Thereafter for some half dozen series he dropped into the habits of the remainder of his group (Jo., Pe., and Ta.) and increasingly overestimated everything with respect to the Var. (Chart V. graph A^2.) Thence on, he *under*estimated, and in an increasing percent, everything with respect to the Var. (Graph A', Charts I and V.) Again, only the persistence in all these figures (A, A^2, and A') of larger measurements in the upper field than in the lower and, usually, of greater estimations in the right than in the left halves (seen finally in "E" of Chart I), gives any grounds for supposing there has been in this subject's work any fundamentally and primarily fixed condition underlying what he perceives as forms.

Now since the changes incident to practice in the case of the two main groups of observers bear so close a relationship to the *method of adjustment*, it is logical to assume that the two distorting elements observed in the study of the "normal field" (Part I, Sec. 1) are but the initial and cumulative effects of *one*

disturbing factor, namely, objective movement in the adjustable or variable line,[10] and that Hu.'s deflections are but indicative of changing subjective attitudes toward this objective disturbance. —It should be said again that care was taken from the beginning to guard against the effects of adjustment. Subjects gave their judgments only when the Var. was stationary, and averted the eyes while the changes dictated were in progress. Nevertheless, their knowledge of *where* the changes were taking place invited to eye shift in the direction of the Var., in order to verify the dictated alteration.

It has been well recognized in previous investigations that the method of "production" or "mean error" is subject to variation in results with changing direction or meridional position of the adjustment factor. As an example, Dr. Rivers[11] noted with both Cambridge men and the Toda tribes the following peculiarities resulting from the method:

> The adjustments from greater to equal tended to give larger measurements than in the opposite direction. . . . The V-H illusion was somewhat larger when the variable was the horizontal than when it was the vertical; about 10.2% in the former case, 6.7% in the latter. . . . "All these discrepancies," says this author, "would be explained if there should exist a general tendency to make the variable line too long when making one line equal to another."

The effects of practice, then, as they developed in the regular procedure of this experiment, it is believed are sufficiently ac-

[10] Dr. Valentine (British Journal Psychology, Vol. V, p. 8), observed the V-H illusion to increase for several subjects as a result of practice, and attributed the matter to the adoption of a more 'mechanical' attitude toward, or a complete yielding to, the *immediate sensory impressions;* and these "sensory impressions" he deemed of a purely retinal sort. These practice effects were among his proofs of a retinal origin of the V-H illusion. But if one accept this theory, then the retina is also solely responsible for the increasing overestimations of all lines in the left half of the field (observed in the work of Jo., Pe., and Ta.), for the equal balance of the right and left halves of a second group, and, further, for *both* an increasing and decreasing series of estimations in the case of still another observer. Practice, then, on this basis proves too much, unless there be a more adequate correlation with retinal facts.

[11] See references, p. 33.

counted for on the basis of the method used. In other words, the subjects have manifested a "mechanical yielding" not to a primary condition of vision, but *to a secondary influence of the experimentation.*

Tests.—A further question remained: Is it possible to overcome the V-H illusion by practice? Mr. Winch[12] reports that with practice in drawing, school children tend to overcome the illusion. In an equal line series of this experiment the phenomenon reached almost a vanishing point for subject Jo. One subject, Hu., was seen to reverse, and three others to increase the usual disparity in the successive series. The problem here is whether or not with continuous practice under uniform conditions accuracy of judgments may be increased, that is, the illusions made to diminish or disappear. There proved to be time for only a single series with a single subject, Ca. The tests were confined to the two lines M 0 and M 90, on which the position of the Var. and St. alternated successively. The time involved was thirty days, with forty judgments at the daily sittings.

Three methods were employed: (1) An abridged normal series, involved merely the "in" and "out" adjustment and the alternate positions of the St. and Var. This was continued daily throughout the course of the tests, even when another procedure occupied a part of the period. (2) In a second series, continuing ten days, the subject was first given "equality" as a norm to which he was to return after a resetting of the Var. (3) For another ten days the method of minimal changes took up a part of the practice period. The adjustable line was moved by stages of one and two millimeters, the subject reporting the point of apparent equality and the first perceptible variation—in either direction—from equality.

Results.—Instead of the hoped for reduction of the illusion or gain in accuracy, there followed these methods merely the settling into fixed habits of judgment. With the Var. at M 0 the illusion became approximately 10%, and at M 90 about 5%, with an average variation of less than a millimeter in any series. Which of the percentages (5% or 10%) is the truer representa-

[12] British Journal of Psychology, Vol. II, p. 220.

tion of the actual illusion as conditioned by the *primary cause,* cannot be said. Both are in close harmony with the data quoted above from Rivers, also that from Wundt pg. 13. Possibly a mean between the two would be our truest estimate of the "primary" facts in this case.

Possibly with younger subjects, such as those reported by Mr. Winch, and also with the kinaesthesis from the hand entering into the practice (as a result of the subject's making his own adjustments), the results might have been different. In Mr. Winch's report, however, the same subjects were not tested at different stages of their practice, but a different group of children for each stage of development. But for this adult, trained, observer, endeavoring to report only the sensory given in each instance, practice in itself has no effect in altering finally the type of his results under any of the conditions here employed. The two type results obtained indicate, as in former series with other subjects, a subjective attitude, possibly of attention, and—something more. The changed attention with the position of the Var. was again insufficient to overcome the illusion in any case, and therefore something more fundamental than was reached by practice effects or attention attitude is indicated.

So far, then, as this experiment affords data upon the subject, practice may be deemed only a *secondary* influence which under given conditions can alter the amount of the various meridional disparities but which does not as yet point to any definite primal cause of those original and persistent disparities that are superior to its influence. It remains for the next section to investigate the basic element in the changing subjective attitude.

5. *The Effects of Attention Attitude*

The preceding analysis of the effects of practice led to an ascription of those results to the objective factor of adjustment, but with the suggested psychic correlate of attention attitude. The purpose of this section is to examine the effects of a voluntary alteration in the direction of the attention, and to correlate such effects with the data of the preceding sections.

Tests[13]

The "norm" for attention (page 9) as given to each subject at the beginning of his work was this: (1) To fixate the center of the field, (2) to think of the St. as the unit of estimation, and (3) to judge when the Var. became equal *to* the St. This, in effect, assigned to the standard line the central, and to the variable a subordinate place in the attention. The problem in the present series is that of testing what results would follow from reversing this attention attitude, that is, from making the variable line itself the focal matter and judging the St. according to it. The final judgment in the former case, in effect, was this: "Now this line at my right [the Var.] is equal to the upper vertical [the St.]"; and in the latter instance, "Now the upper vertical [the St.] is equal to the right horizontal line [the Var.]"

Two series were given: (1) the regular normal, i. e., the attention on the St., and (2) a repetition of the same objective procedure, but with the subjects' attention on the Var. The Var. throughout remained at M 0. The observers were Jo., Pe., and Hu. The time was immediately before the close of their two to four quarters' service in the experiment. All the practice effects, then, that have been described, were fully developed.

Results.—Table VII gives the results in full. The following are the principal facts.

Subject Hu.—(1) In the "normal" series, attention on the St., this subject continued to give the negative results that had characterized his work from the beginning of the control tests. For example, the illusions of the upper and lower verticals, this series, were respectively —10.05 mm. and —21.15 mm. (2) With attention on the Var. (M 0) the positive illusions, characteristic of his earliest work reappear, not only in the vertical lines but also in the diagonals of the upper field. The overestimations of the two verticals now become 3.2 mm. and 2.15 mm. The increase in the apparent length of the upper vertical, thus, is 13.25 mm., and of the lower, 23.3 mm.

[13] The attention series was brief because coming at the end of the service of busy subjects pressed by their own student work, and because also its apparently high significance was not realized until the evaluation of the data some months later.

Now if the voluntary direction of the attention toward the horizontal line (the Var. in this case) produces the usual positive V-H illusion while the direction of the attention toward the vertical (here the St.) negatives that illusion, then it seems in this case at least, that *the line held in the focus of the attention tends to decrease in its apparent length or to be underestimated with respect to that one more marginally viewed.*

Subject Pe.—(1) In the case of this subject it is the normal series (attention on the St.) that shows the greater variation from the usual normal results in the work next preceding this. The variation, however, is confined to the significant lines M 90 and M 90′ (upper and lower vertical), all other results falling well within the average variations. The illusions for the upper and lower verticals are respectively +18.85 mm. and +12.9 mm. (2) With attention on the horizontal (the Var.) these illusions become respectively +25.8 mm. and +19.45 mm.

Again, as for Hu., the voluntary direction of the attention to the horizontal line results in an increase in the apparent length of both the upper and lower verticals. Here these increases are respectively 6.95 mm. and 6.55 mm. Attention to the vertical does not in this case destroy or reverse the V-H illusion, but it does noticeably diminish its amount. If, then, attention to the horizontal line *increases* its usual *under*estimation with respect to the vertical, or attention to the vertical *diminishes* its corresponding *over*estimation with respect to the horizontal, there is harmony with the results of Hu. in the evidence that *the line held in the focus of the attention tends to diminish in its apparent length, or to appear of less extent than the same line when more marginally attended.*

Subject Jo.—All differences shown by this subject in the two series are well within the average variation of the usual normal series corresponding to this period of his work.

The introspections of the three subjects measurably account for the differences in their results. When the norm for attention was first given, when it was restated in common with all other norm conditions at the beginning of the "Control" tests, and when at last it was emphasized and made the basis of this

final series of those tests, each of the subjects made enlightening remarks. In the beginning, their comments, like those of all other observers, were upon the difficulty of steady fixation at the center. For a time their chief problem was to gain comparative control in this respect.—In doing this they reduced their average variations not only within each single series but also between the "in" and "out" series.—As to the balance of attention between the Var. and St. lines, the subjects were required only to answer *in terms of* the St. that is, to state that the Var. was too long or too short with regard to it. This at first was supposed by the operator to be sufficient guarantee that the attention was chiefly directed toward the standard, as required in the "norm."

But at each of the two restatements of the attention norm this supposition was proved a mistaken one. Jo. commented in each instance that the condition was very hard to fulfill, that he felt sure his attention fluctuated between the two lines and had done so in the other series. In the last tests (Attention series) he was clear in the statement that he could detect no difference in the two attention attitudes required. His results bear him out; no appreciable difference is found in the two series.

Pe. made comments very similar to those of Jo., but in the Attention series apparently his intenser efforts at control were effective upon the first two lines of the group—the upper and lower verticals—for here the amounts of the overestimations are reduced from what they had grown to be with practice to something nearer their averages in his earlier normal series.

Hu. was himself experimenting at this time, in animal psychology, and hence keenly attentive to any changes of conditions; he also had remarkable powers of concentration in any given task. It is therefore not surprising that the change of method here impressed him especially. His comments at each restatement of the norm were in the way of questions to make sure he understood; a knitting of his forehead showed the intensity of his effort to fulfill the requirement.

Now, granted that these observations are true and that the results of this voluntary attention series are valid as a basis

for interpretation of earlier data, then the following seems a plausible account of what had taken place in the general Practice stages.

(1) In the preliminary stage all subjects (like Pe. and Jo.) must have allowed the attention to drift naturally between the two lines compared. (Minor fluctuations in the results, throughout the experiment, *may* have been due largely to such fluctuations of the attention.)

(2) Since in a part of this preliminary period the three subjects were alike in showing an overestimation of all other lines with respect to the Var., the attention must have been more frequently or more strongly attracted by that line than by the St., thus giving to it rather than to the St. the real focal place.

(3) The tendency to overestimate all other lines with respect to the Var., for a time increased with practice in the case of all subjects. (See page 26.) This on the above basis would signify an increased yielding to the involuntary attention drift as influenced by the adjustment. This inference is corroborated by these particulars: (*a*) For Hu. the increase with practice was arrested at an early date. With his first clear understanding of the attention norm (St. focussed), he began to reverse the type of his results. (See Table IV, Ocular Position series.) This reversal, i. e., the negative illusion, continued and increased with practice until in the voluntary attention series the Var. is again focussed. With this the earlier type of results follows. (*b*) For Pe., whose practice was less regular, the increased illusion in the vertical was less marked than in certain other lines. But the very large overestimation noted is unarrested until, in the final Attention series, under stress of renewed urging he shows in the first two lines of the series (M 90 and M 90′) a tendency to smaller disparities more typical of earlier work. And this decrease occurs when the attention supposedly is upon the St. (*c*) For Jo. there was no noteworthy arrest of the tendency to enlargement of the illusion from practice. Apparently whatever was his attention attitude in the beginning continued with increasing effectiveness until practically the last; and this best accords with his introspections. Since these results are in har-

mony with those of Pe. and Hu. when the attention was "adrift," and also in harmony with what the latter evidenced with the voluntary attention upon the Var., we must conclude that the Var. line must have been the controlling influence upon the attention in Jo.'s case also.

(4) All the above is borne out by the fact that in other series, where the position of the Var. was altered, there followed in each case the increased overestimation of lines in the field opposite the Var. If this were due to a stronger "pull" of the attention toward the Var. than toward the St. line (and is there any reason why the opposite, for instance, should be true?) then again it is the line held in the focus of the attention that tends to decreased apparent length in comparison with the one more marginally viewed.

(5) In complete harmony also is the additional fact that for those subjects in whose case the position of the Var. was altered with each successive series, there did not follow with practice an increase in the average amounts of the illusions.

So far, then, the results of the voluntary attention series may be said to corroborate the conclusions of Sec. 5, to the effect that practice results are attributable to a subjective attitude relative to a chance element of method, namely, the objective movement in the variable line.

Comparison with Data of Earlier Experimenters

Rivers,[14] as noted above, points out the indication of *"a general tendency to make the variable line too long when making one line equal to another,"* and the relation of this to the changing amount of the V-H illusion with alteration in the position of the variable. The work of practically any investigator of a similar problem illustrates the same point. There is in this matter perfect accord with the data of this experiment (Part I, Chart I). If there exist also, as evidenced by this voluntary attention series, "a general tendency" to overestimate the line in the margin of the attention relative to that in the focus of the attention, then we must conclude that in all these recorded

[14] See p. 62.

cases the Var. line has thrust itself into the focus and the standard more into the margin, and hence attention, or attention attitude, has influenced the results.

Valentine suggests the greater difficulty of *attending* "to objects in the vertical than in the horizontal periphery, owing to our greater practice with the widely extended horizontal field," as a possible cause of the V-H illusion, but gives no direct proof of this or of his suggested basis for the increase of that illusion with practice. (See footnote, page 62.)

Stevens' experiments[15] offer data the most closely allied with that of the attention series here. He found that peripheral extents were overestimated with respect to similar extents in the focus of vision. Now the focus of visual attention and the focus of the visual field are not *necessarily* the same, but ordinarily they are; likewise with the margins of the two. The identification of the data of this series with that of Prof. Stevens implies that in this experiment *with each attention shift there was a corresponding eye shift.* If the latter may be granted—and it is not discordant with the introspections at least in the earlier series—then there is exact correlation of facts. At any rate we do know (1) that the margin of the visual field (where Prof. Stevens notes the overestimations) is *attended to with greater difficulty* than the focus of that field, and (2) that in this experiment the line left voluntarily in the margin of attention tended to increased estimated length in comparison with the line focalized. To this extent there is undoubted harmony. Upon this basis one may examine the ocular structure for conditions that would render marginal vision different from the focal and inquire if those conditions are greater in extent in the vertical than in the horizontal dimension of the field. For *explanation,* unless one is satisfied with a central—Attention—theory, must push farther than the experiments of this investigation have been able to go.

[15] See p. 12.

Summary of Part III

(1) The phenomena of the normal field are unaffected by the experimental changes introduced in (a) Ocular Position, (b) Bodily Position, and (c) Objective Contour.

(2) Practice does change, and very appreciably in some cases, the amount of meridional disparities. The changes connect themselves closely with those involving the adjustment factor. Practice, however, does not avail, in the given tests, to overcome or appreciably to reduce the illusions.

(3) Attention attitude is effective in altering the type of disparities in the visual field. Apparently this may account for those changes growing out of "adjustment" and "practice," possibly even the minor variations as well.

(4) The specific effect of voluntary attention attitude apparently is to diminish the subjective length of the line focalized relative to a similar line more marginally viewed.

(5) The data of the experiment, as so far reviewed, do not conclusively reveal the structural basis of the phenomena. It is found necessary, therefore, to supplement with facts of histology.

THEORETICAL EXPLANATION. RETINAL STRUCTURE

1. *Summary of Theories*

The oldest explanation of the vertical-horizontal disparity, and kindred illusions, is, according to Wundt,[1] the imagination-judgment theory. This in various of its forms is advocated by Helmholtz,[2] James,[3] and Lipps.[4] The first two suppose the usual allowance made for perspective foreshortening of squares seen in the horizontal plane to be the basis of the habit of over-estimating the height of a square seen in the upright position. Lipps makes the aesthetic judgment the cause of such disparities; that is, the "habit" of *perceiving movement* in lines—a striving upward or erection in vertical extents and a corresponding contraction of the horizontal dimensions of figures—results in the relative over- and underestimations in these directions. The three agree in supplementing the retinal impression with data of a central nature in order to account for the illusion. The contour theory of Külpe[5] assumes that the image of the given figure is supplemented by the influence of additional data from the environment; and thus it also in effect becomes a judgment theory.

Now aside from the fact that the retinal sensory data seem abundantly adequate to account for visual space perceptions, including the meridional illusions, there are other factual evidences that militate against the judgment hypotheses. In the first place, the contour theory seems to lose the case upon experimental grounds; and the aesthetic (?) perception of movements in lines may easily be traced to eye movements, or attention

[1] Die geometrisch-optischen Täuschungen, S. 157 ff.

[2] Handbuch der physiologischen Optik, S. 702.

[3] Psychology, Vol. II, pp. 264.

[4] "Aesthetic Faktoren der Raumanschauung," in Beiträge zur Psych. und Phys. der Sinnesorgane; Festgruss zu Helmholtz.

[5] *Suggested*—Outlines of Psychology, pp. 365-366.

phenomena. In the second place, perspective—even if the observer were not *sure* in giving his judgments that he sees the experimental figure in a plane—is not wholly deceptive. One recognizes a square placed diagonally *as*—a square in the diagonal position. Again, one may see a drawing at will as a plain surface or as tridimensional. But the illusions of the vertical-horizontal type are practically unescapable. Furthermore, it is found that young children and primitive peoples (Todas and Papuans)[6] manifest a larger V-H illusion than do civilized adults. It is difficult to understand how these (especially the savages) can have been confronted with a sufficient number of squares and rectangles (printed pages, carpets, etc.!) to have built up so nice a set of *habits* of judging as the perspective theory of James or Helmholtz presupposes. Such explanations for the illusion are open, finally, to the objection that they ascribe to judgment and habit (factors that are supposed to correct our sense impressions and assist in adaptation to objective realities) a process that is fundamentally misleading.

A second group of theories assumes a relationship between asymmetries of the bulb and the given illusions. As an extension of the Kundt-Hering theory,[7] namely, that of the estimation of the distance between two points by the chord rather than by the arc between their retinal images, there is needed but to presuppose the requisite differences in the vertical and horizontal dimensions of the bulb to account for the V-H illusion along with the illusion of unfilled space [Hering] and the illusion bearing Kundt's name. (See page 12.) But apparently this presupposition is not established by the facts of anatomy. Moreover, both Wundt and Delboeuf show that a space divided by a single point is underestimated with respect to an open space of the same extent, thus:

. . . .

This would apparently invalidate the whole theory of the estimation of space by retinal chords.

[6] See references, p. 33.

[7] Poggendorf's Annalen, cxx, S. 118 ff. Hering, Beiträge zur Physiologie.

1 S.65 54.

PSYCHOLOGICAL MONOGRAPH NO. 101

ERRATA

2. The line of dots on page 73 should have been spaced as follows :

. . . .

Asymmetry of the lenses, astigmatism, was considered on page 32. In the past it frequently has been suggested, and as often abandoned, as a possible cause of the V-H illusion. Wundt[8] long ago pointed out that (1) astigmatism varies greatly with individuals, is often entirely absent, while the illusion is universal, and (2) compensatory lenses that correct astigmatism do not cause a disappearance of the illusion.

Wundt's own theory, as noted, is built upon the known asymmetries in the size of the muscles moving the eye in the vertical and the horizontal directions. He dismisses the "judgment" theories as not only the oldest, but also the most unscientific of the hypotheses. But it is difficult to see how the blending of sensations from disparate senses (retinal and muscular), as called for in his theory, ever came to be without the activity *at some time* of conscious or unconscious judgment. If this blend or fusion is a conscious, ontogenetic, acquirement, then his is a judgment theory; if it came about phylogenetically, there is left the puzzle of how far Wundt is at this point from being a nativist. Indeed both Wundt and James apparently contradict their general principle of explanation of visual space perception in their accounting for the normal space illusions. The experimental aspect of this discussion has been stated, pages 53-56.

In all the above theories, apparently, nativists and empiricists alike lose sight of the retinal image as a possible factor of close correlation lying directly between the objective extent on the one hand and the subjective estimation on the other.

Recently, however, the advocates of a purely retinal theory have grown in numbers. Witmer[9] ascribes a *part* of the spacial illusions (e. g., filled versus unfilled space) to variation in the degree of the "physiological excitation" of the retina; other illusions, e. g., the V-H, to variation in difficulty of eye movement. Stevens,[10] whose work on foveal and peripheral differences of estimations is so significant of retinal conditions, considers the overestimations he finds in the right cyclopean field

[8] Die geometrisch-optischen Täuschungen.
[9] Analytical Psychology, p. 92.
[10] Psychological Review, Vol. XIX, p. 30.

must be due to "some yet unknown anatomical or physiological condition peculiar to the left corresponding halves of the retinae in consequence of their connection with the left hemisphere of the brain." This, however, does not account for the more significant foveal and marginal differences, or for the vertical-horizontal disparities. At least there is no apparent connection, to the writer's knowledge. Valentine[11] proceeds by a process of elimination (i. e., of muscle strain, of astigmatism, and of aesthetic appreciation) to the conclusion that the retina itself must be the source of the meridional disparities. As to either the physiological condition of the retina, or the "obscure psychological factor at work here," he has no final explanation except to suggest, very aptly, the possibly greater difficulty of *attending* in the vertical than in the horizontal dimension or "that those conditions, whatever they may be, which give rise to variations in the apparent size of objects as seen by different parts of the periphery of the retina, differ from the conditions at the center more in the vertical direction than they do in the horizontal direction."[12]

2. *Retinal Structure*

It is the purpose here to examine the retinal surface, both as to its physical characteristics, or form, and as to its anatomical detail, with a view to discovering what, if any, correlations may exist between the data found and the functional facts revealed by this and similar experimentation.

[11] British Journal of Psychology, Vol. V, p. 327 ff.

[12] Dr. J. W. Hayes finds that, in foveal vision, of two equal luminous bodies in a vertical relation to each other, the lower is frequently judged to be of the greater magnitude. While the correlation of this illusion with definite structure, or with other functions, is not yet evident, this author concludes that its restriction "to foveal vision indicates its dependence on functional and hence on structural peculiarities of this region as contrasted with the rest of the retina." *A Horizontal-Vertical Illusion of Brightness in Foveal Vision,* Psychological Monographs, Vol. XX, No. 1.

James, Stumpf, and Külpe are well known among the older psychologists who "conclude . . . that the retinal impressions are from the first endowed with a spatial predicate"; but they are very slow in ascribing to the retinal impressions the responsibility for the illusions of space.

The maintenance throughout the experiment of the primary fixation at the center of the field makes possible a fairly close approximation of the retinal areas affected in each series.

(1) *Physical Aspects of the Retina.*—Is there anything in the physics of the optical organ that may interpose to prevent uniform correlations between retinal magnitudes and objective magnitudes? In other words, is it or is it not a certainty—as many writers appear to assume—that equal objective extents mean invariably equal retinal extents, no matter in what part of the field, foveal-peripheral or meridional, the image may fall? Such an inquiry must necessarily be faced, whether or not its conclusions aid in explaining the given phenomena of visual space. The question as it is affected by meridional differences, i. e., irregularities of curvature in bulb or lens, has been fairly dismissed in the preceding discussion. But the matter of foveal and peripheral differences has yet another aspect. If the retina were a perfect sphere, and concentric to the nodal point, and the refractive index were identical throughout the eye, then exact equality in size of images from like objective cause, no matter what the direction of its location, might reasonably be expected. But the eye departs from these ideal conditions. The refractive index changes with each angular remove from the line of fixation, and the retina is not a perfect sphere. The completion of the retina would pass through the crystalline lens; the *ora serrata* is about on a plane with the nodal point. The refraction at the margin tends to bend the image back toward the center of the field, while from the forward location of the nodal point arises the physical necessity that the images falling marginally (nearer the node) occupy a less space than those from a like objective extent but impinging upon the central part of the retina.

But, so far from finding in these dioptrical conditions an explanation of the foveal-peripheral differences of vision pointed out by Prof. Stevens and apparently evidenced by this experiment, there is, on the contrary, an added difficulty. The real causal condition—whatever it may be—must be of sufficient

potency not only to produce the overestimations at the margin of the field with respect to the foveal region, but to overcome in so doing the counteracting influence of the diminished sizes of the images at the margin.

(2) *Histology of the Retina.*—The arrangement of the retinal elements is thus stated by Ladd and Woodworth:[14] "In general the rods are more numerous than the cones. The distribution of the two elements is different for different parts of the retina. In the yellow spot [macula] only cones appear, but these are of more slender form and of increased length, so that not less than one million are supposed to be set in a square 1/10 inch; while not far from this spot each cone is surrounded by a crown shaped border of rods. Toward the *ora serrata* the cones become continually rarer."

Apparently, since the slenderness of the cones and their exceeding compactness are mentioned as a peculiarity of the macula and especially of the foveal area, one is to infer that marginally the tendency is toward a general decrease in the number not of cones only but of all retinal elements per unit of area. At least we can be certain that the massing of the *cones* falls in the macula, while a larger and larger proportion of rods prevails towards the margin.

Moreover, since the cones decrease in *length* from the fovea toward the margin, the surrounding or "crown-shaped border" of rods, may be supposed likewise to diminish in this dimension. This is supported by the statement of Gray[15] that the retina "gradually diminishes in thickness from behind forward."

A further differentiation between the marginal and macular structure arises from this peculiar distribution of the rods and cones. This is in the matter of the neural connections. We are told by various authorities that the cones are usually connected singly with one bipolar cell each, while the rods are connected in groups of a half dozen or more to a single bipolar cell.[16] The

[14] Elements of Physiological Psychology, p. 193.

[15] Anatomy, p. 860. (Edition 1893; Pick.)

[16] See Quain: Anatomy, Vol. III. Pt. 2, p. 232. Also Fig. 169, p. 238. Angell: Psychology, Fig. 49, p. 141.

neural equipment, then, of the center of the field is superior to that of the margin.

These details of structure seem sufficient to necessitate *some* differences in focal and marginal space estimation. As a matter of fact, and whether or not there be a cause and effect relationship, the correlation actually found to exist, according to Prof. Stevens' data, is as follows: comparative underestimation accompanies the superior equipment of the central retina, while corresponding overestimations are coupled with the lessened efficiency of the margin.

It is desirable next to inquire the relative extents of the marginal and macular areas in the vertical and horizontal meridians of the retina. Cunningham,[17] and apparently all authorities who mention the matter, state the longest dimension of the macula—the area of compact conal structure—to be the transverse or horizontal diameter. Helmholtz[18] gives the following measurements from Kölliker:

Horizontal diameter of the macula	3.24 mm.
Vertical " " " "	0.81 mm.
Diameter of the fovea centralis	0.18 to 0.225 mm.

Whether the last named dimensions mean that the fovea also is an oval is uncertain; the presumption is otherwise.[19] But clearly the macula is a slender oblate oval, one-fourth as wide as long. This means that the area of closely crowded elements extends about four times as far horizontally as it does vertically, or, that the marginal conditions of structure, whatever they may mean of deficiency or inferiority of equipment, approach more nearly to the center in the vertical than in the horizontal radius. Again, whether or not there be a cause and effect relationship, the actual correlation of the data is as follows: The line having the more extended macula, or superior equipment as to structure, is the one which corresponds to the greatest comparative underestimation in the visual field, namely, the horizontal, while the vertical dimension which shows the widest extent of marginal

[17] Textbook of Anatomy, p. 815.

[18] Handbuch der Physiologischen Optik, S. 37.

[19] Parsons (*An Introduction to the Study of Color Vision,* 1915, p. 9) says: "The foveal region is an elliptical area with the long axis horizontal. The long axis measures about 0.3 mm., the vertical 0.2 mm."

structure likewise corresponds to the visual line of greatest comparative overestimations. This is in entire harmony with the above correlations of general foveal-marginal structure with facts of focal-peripheral vision.

The third inquiry arising is, do there exist also inequalities in the distribution of the rods and cones in the upper and lower vertical or right and left horizontal areas of the retina, such as would correlate equally well with the disparities found in the corresponding parts of the visual field? Further, does the general ovoidal arrangement of elements prevail in the medial and marginal parts of the retina as well as in the macula itself—as would be required for a correlation, on the same basis, with the data of overestimated diagonal meridians? To these questions the histologists consulted give no answer. An inference from a related function is, therefore, all that is available at this point.

(3) *Structure Inferred from Known Function.—Two-Point Acuity.*—Apparently no theorist has been bold enough to suppose that the matter of visual two-point acuity is dependent upon imagination, aesthetic judgment, practical judgment, influence of objective contour, or even of eye muscle strain,—or upon any fact or feature other than that of the retina itself with its peculiar arrangement of elements. If, then, the function of two-point discrimination—or other function peculiar to the retina alone—has been more thoroughly worked out in the various areas of the field than has the minute detail of cone and rod distribution, it seems legitimate to refer to these studies for some inference concerning these further facts of structure so desirable here. At least it may be seen if the two functions of point discrimination and spacial estimation correlate with each other as well as either apparently correlates with the structure itself. It will be the purpose, then, in the following paragraphs to carry forward a dual comparison, namely, that of the two-point acuity data both with the structural detail above cited and with the corresponding data of this experiment.

Sanford[20] says: "The discriminative power of the retina falls

[20] Experimental Psychology, p. 106.

off rapidly in all directions from the *fovea*—more rapidly above and below than in the horizontal direction." There is here an evident correlation with the facts of structure, i. e., with the ovoidal form of the macula and the more extended "margin" in the upper and lower vertical than in the horizontal borders. There is, further, our first indication of an existing correlation between comparative underestimations and a high visual acuity, and, vice versa, between overestimations and a low acuity.

Poschoga[21] found the discriminative power decreased in successive removes from the pole of vision, and certain of his figures show a markedly lower acuity in the upper vertical than in the right horizontal meridian. These facts again correlate the foveal-marginal and vertical-horizontal differences of space with the discriminative differences on the basis named above, as well as with the known facts of structure.

Wertheim[22] has worked out in much detail the problem of

Fig. 7.

After Th. Wertheim: Ueber die Indirect Sehshärfe, Zeitschrift für Psychologie und Physiologie der Sinnesorgane, VII. B., S. 185.

[21] Psychologische Studien, B. VII, S. 384 ff.

[22] Zeitschrift für Psychologie und Physiologie der Sinnesorgane, B. VII, s. 172.

acuity in indirect vision. His observations were made upon his own left eye, and his results—which he says are in harmony with those of numerous investigators whom he cites—are presented in a graph repeated here as Fig. 7. The points on the different meridians having equal acuity are joined by curved lines. The main facts indicated and which he states in his discussion are as follow: The visual acuity diminishes in the upper field most rapidly, somewhat less rapidly in the lower field, still more slowly toward the medial side, and the very slowest laterally (outer side). However, within a radius of some 15° from the pole of the field—the area with which this report is immediately concerned—the curves apparently mark a reversal of this lateral-medial relationship.—That there is a quick descent in acuity from the fovea marginally in stages indicated by the curved lines is shown by the following statement of the proportions. Representing the acuity (*Sehshärfe*) of the center by 1, the first curve drops to 0.333, the second to 0.2, and the third to 0.143, and so on to the outer one, whose acuity is represented by 0.026.

It is further apparent from the diagram that in the function of discrimination the *ovoidal arrangement* of the "zones" *persists from the macular region to the margin of the field.* Since high acuity is generally correlated with number of retinal elements, we have, then, in this functional data a strong indication that the number (or efficiency) of the retinal elements would be found to diminish more rapidly in the lower retina, which views the upper field, than in the upper retina, and, in the extra macular region, more rapidly in the vertical than in the horizontal dimension. In the absence of histological data, which would be extremely valuable at this point, the inference as to structure may be permitted us. On the other hand, there is a clearly marked and unmistakable correlation between the two functions of discrimination and spacial estimations, and in perfect harmony with the fact cited above, namely, that where discrimination is low there is high overestimation and where discrimination is acute there are comparative underestimations. In other words, the acuity chart and the typical field graphs show an inverse relation

as to size of parts. The former has its greatest diameter in the horizontal dimension, its least in the vertical, and its lower vertical extent is greater than its upper, while there is a variation with location in right and left parts. The latter—for the majority of subjects—has its least diameter in the horizontal, its greatest in the vertical direction, is more extended (usually) in the upper vertical radius than in the lower, and is less certain in the relative proportions right and left of the center. The diagonal radii likewise take corresponding medial places between the vertical and horizontal lines in the radial disparities of the two graphs. Furthermore, in passing from the central to medial parts of the field there is shown, in the discrimination chart, a variation of outline indicative of a possible correlation—were the two charts in each instance made for the same eye of the same individual—of the two functions in the corresponding concentric segments, as well as in the larger field outline.

Finally, it is entirely conceivable, from these concentric zones of acuity that the slightest shift of *attention,* with a corresponding eye shift, would tend to alter the position of the images of the given lines relative to the retinal parts, and hence relative to the number or efficiency of the elements affected.

On this basis, then, of structure known from histology and of structure inferred from closely correlated function, it is possible to trace a direct *correspondence* of the peculiarities of meridional space perception with the anatomy of the organ most closely concerned with vision, namely, the Retina.[23]

3. *Detailed Correlations*

The angular extent of the retinal space covered by the normal St. of this experiment was 6° 12′, which approximates the ver-

[23] Wundt (Outlines of Psychology, p. 139) makes an attempt to dissociate the functions here correlated, ascribing the one to the retina, the other to the eye muscles. His basic assertion is that two points, as soon as they are distinguishable at all, "will appear just as far apart in one region [foveal or peripheral] as in the other." This is at variance with Professor Stevens' more recent results. Very simple tests, also, such as passing two pencils, held a short distance apart, from the margin around to the center of the field, cast a doubt upon the statement, and lead one to question if Wundt's utterance is not somewhat dogmatic.

tical limits of the second of the acuity curves of the Wertheim chart. Combining these in the same figure and adding an approximate outline of the fovea centralis and the macula lutea, reduced to the same scale—one-fifth the objective size of the standards—gives a figure upon which may be based a comparison

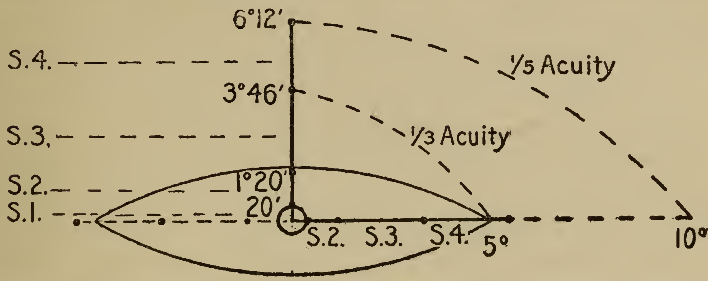


FIGURE 8.—The small circle represents the fovea centralis; the oval, the macula lutea; the solid vertical and horizontal lines are the normal standards of this experiment (the dots indicate the extent of the shorter standards, S. 1, S. 2, S. 3, and S. 4); the broken curves represent the Wertheim lines of acuity. All are reduced to the same scale. The extents of the retinal angles included in the different spaces are indicated in degrees and minutes.

of values. (Figure 8.) Exact mathematical accuracy is not possible, since the functional and histological studies could not be made upon the same eye. Nevertheless, the results of the authorities quoted have been sufficiently verified by others in their field[24] to be accepted as typical for the human eye in general, and to afford a reasonable basis for the comparison of data. It is seen that this entire experiment has been taken up with a very small area in the central part of the field as presented by the Wertheim figure.

(1) *The Vertical-Horizontal Illusion, Normal Standard.*—The radial extent of the normal standard in retinal terms (see page 5) is 1.727 mm. The transverse radius of the macula (½ of 3.24 mm.) is 1.62 mm.; the vertical radius (½ of .81 mm.) is .405 mm. The image of the St. in the horizontal position, then, when the inner end is fixated, overlaps the macula by *.1 mm.;* in the vertical position, the difference is *1.32 mm.* If spacial estimations are to correlate with retinal structure, the

[24] See bibliography cited by Wertheim.

principal surprise here is not that there should be a V-H illusion, but that its percentage should be no greater than it is usually found to be in the normal eye.

It is true that the macula (according to Kölliker) has a width only one-fourth its length—a difference of 75%—whereas the V-H illusion is seldom reported as more than 25% of the St. length. But histology has not answered as to the actual *rate of decrease* of retinal structure or efficiency, in the various radii, beyond the macula. A comparison, then, with the acuity data is more helpful. It will be noted by the chart (Fig. 8) that the image of the horizontal St. of 140 mm. reaches a point measuring about 1/3 the efficiency of the center of the field, while the vertical line of the same length terminates in an area of about 1/5 efficiency. The difference is 2/15—interestingly near the Wundtian ratio of "1/7" in the comparative enlargement of the vertical line over the horizontal. If, then, two-point acuity correlates with the structure of the retinal surface, it seems reasonable to suppose that another function in so close correlation with the acuity data might be equally dependent upon the same anatomical conditions.

(2) *Overestimation of the Diagonal Meridians.*—The fact that the Wundtian graph (Fig. 5) is not a perfect quarter oval, but shows a sudden bend outward from the horizontal line, and the further fact that the graphs of this experiment (the "field types") have very "square" corners, are each in perfect harmony with the former indications of a close inverse correlation between two-point acuity and space estimations. If the same structure *should* be the basis of each of these two visual functions, then the triple correlation as evidenced by the graphs (Chart I, also Fig. 5 and Fig. 8) is as follows: For each successive angular remove of a given line from the horizontal meridian of the retina there follows a decrease in such structural efficiency as marks the macula, a decrease in acuity, and a corresponding increase in the overestimation of the linear extent, until the maximum of these conditions is reached in the vertical line. Indeed, if the structural hypothesis be true, then the Wundtian graph (Fig. 5)

indicates a very tapering oval in the macular form, with perhaps an especially marked prolongation in the horizontal meridian.

(3) *Foveal and Peripheral Differences of Meridional Disparities.*—Figure 8 above indicates that part 1 (S. 1) of the St. corresponds in retinal extent quite closely with the fovea itself; that part 2 (S. 2) in the vertical direction practically reaches the limits of the macular area, and that parts 3 and 4 (S. 3, S. 4) of the same line, accordingly, are outside this area; and that, on the other hand, the corresponding parts 2, 3, and 4 of the horizontal line fall wholly, or almost wholly, *within* the macula.

If, then, Figure 7 is as truly indicative of structure as it is of the function of acuity, the "triple correlation" yet holds, and it is not a matter of surprise that there was found in Part II of this experiment a tendency to increase the percent of the V-H disparity as the St. and Var. lines were moved by successive stages from the center toward the margin of the field. That segment 3 should so often have shown a higher percentage of the illusion than even segment 4 is peculiarly interesting in view of the fact that in M 0 this segment lies *wholly* within the macula and thus may be presumed to have great structural advantage over the corresponding segment which in M 90 lies almost wholly in the extra-macular region. Shifts in the attention, however, might readily vary the results here as well as in segment 1, where the negative illusion is recorded for some subjects and the positive illusion for others; for the tiniest movement of that square one-tenth inch into which one million elements are crowded might vary by several hundreds, or even thousands, the number of elements affected by the given stimulus. Furthermore, there *might* be unique variations in the exact pattern up and down and right and left in the arrangement of those elements in different eyes, and hence individual differences in results. That the percentage, positive or negative, was always high in this central segment is not so significant as might seem, since any appreciable error would be large in proportion to so small a base—7.5 mm. (See Table III.)

The Figure 7 (two-point acuity) as a whole correlates inter-

estingly with the statement of Fischer that the eye has different *"Maasstäbe"* in the different meridians, as well as with the data of this experiment indicative of a variation in the *"Maasstäbe"* (measuring units) in passing from the fovea to the periphery along the various meridians. (It will be noted from the Figure 7 that the acuity curves are not absolutely uniform or parallel. How in this case it can be supposed that the acuity phenomena are due to the retina alone while such closely correlated data must be attributed to another sense, the kinaesthetic, it is not quite clear to the writer. Were the Weber-Fechner law *established* in the phenomena of space estimation, as Münsterberg concludes from most irregular data, then the muscular theory might be expanded to include the phenomena, though it would be unnecessary. But the data of Chodin, and the data of this experiment *seem,* as stated, to evidence a possible *irregularity* in percentage of the illusion with different lengths or segments of the standards. This irregularity, whether a phenomenon of the attention only or an inherent feature of the normal, fixed, conditions, can be reconciled with a purely retinal hypothesis. (See topics 6 and 7 below.)

(4) *Disparities of Upper and Lower Vertical Extents.*—Five of the eight regular subjects of this experiment overestimated the upper vertical meridian with respect to the lower; two others showed considerable variation in the matter; while one, Jo., in 75% of a long series of tests reported the lower field greater than the upper. The Wertheim chart, Fig. 6, agrees (on former basis) with the majority, by showing a higher degree of discrimination in the lower field—viewed by the upper half of the retina—than in the opposite area. If, again, this chart is indicative of the *usual* arrangement of retinal structure and the correlations here cited are valid, then it is to be expected that in the majority of cases, as Delboeuf [25] has reported, the upper field will be seen larger than the lower. We have no direct knowledge of the differences in relative width of the upper and lower parts of the macula; but *if* the two closely correlated visual functions may be *thought of* as dependent upon the same

[25] Page 12.

visual structure, then there is here the double inference that the macula must be narrower in its lower than in its upper extent, as divided by the horizontal line cutting the central point of focal vision. To account for those cases—for example Fischer,[26] and subject Jo. of this experiment—where the lower field is pronouncedly and repeatedly seen larger than the upper, one must suppose either a marked difference of *attention habit* resulting in a more or less fixed ocular position (see Part I, Sec. 2, series 3), or else a primary difference in structural arrangement in the eyes of the different individuals. The fact that the illusion of the figure 8—readily seen by inversion, thus, 8—is common to the race is indicative again of the truth of the Wertheim data and of the general correlations that have been based thereon.

(5) *Disparities in the Right and Left Horizontal Extents.*—The Wertheim graph is of the monocular field, left eye. Within a radius of 15° from the pole, apparently discrimination is greater in the right field—viewed by the temporal retina. Beyond this area the difference is reversed. The experiments of this paper fall within 7° from the pole. The line halved by the subjects of Kundt,[27] who showed an overestimation in the outer field (nasal retina), had an angular extent on either side of the pole of about 9°. It is not surprising to find superior discrimination in the temporal retina in the first removes from the center. The color fields have for some observers a much wider extent in the temporal than in the nasal retina. The temporal sides of the two retinae attend to the section of space immediately in front of the individual, that is, in the binocular field, and to the finer objects of near vision. Beyond the area of distinct binocular vision, it is the nasal retina which guards the outer horizontal margins, and *it* is provided with the wide areas of acute vision.

[26] Fischer not only saw the lower vertical greater than the upper, but he overestimated the inner with respect to the outer half of a given radius. The fact that he was one-eighth shortsighted raises the question if the second phenomenon may not in his case be due to dioptrical rather than to structural conditions. (See page 76.) At any rate, the data is no more easily explained by the muscle-strain hypothesis, for instance, than by a retinal theory.

[27] Page 12.

But the linear extents used in the present experiment nowhere test the differences in spacial estimation in parts of the extreme margin of the field. The correlations are confined to the central area, where there is nothing contradictory in the indicated structural arrangement (Fig. 7) to the statement of Kundt that in monocular vision the outer part of the objective field is overestimated with respect to the inner. Indeed there is again a proof from the work of these two authors (Kundt and Wertheim) that there must be a correlation of the two functions of discrimination and spacial estimation, the finer discriminations being coördinated with the comparative underestimations.

On this basis one should expect a nice balance in binocular vision between the right and left halves of the field. Such a balance or equality of estimations was approximated by several subjects of the shorter series of this experiment, in which the position of the variable line was altered in successive tests. Nevertheless, in those series affording the nearest possible elimination of the adjustment factor (see "Equal-Line" series, Part I, Sec. 2) there persisted still some individual differences in the characteristics of right and left fields. Reports from other investigations are alike indicative of individual differences in this respect. Stevens[28] says the right field is overestimated with respect to the left. Münsterberg[29] asserts the left is overestimated with respect to the right. Külpe[30] says a distance to the left "generally" appears greater than a distance to the right. In the "Eight-equal-line" series of the present work, four of the five subjects saw the right horizontal line as the shortest of the radii, yet two of these later indicated a reversal of this right and left difference.

As to the persistent individual differences, one of two possible hypotheses must needs be adopted to complete the correlation with the discrimination, and the inferred structural, data. There must be presupposed the existence either of a more persistent attention attitude on the part of the subjects concerned, or, an

[28] Psychological Review, Vol. 15, p. 69 f.; Vol. 19, p. 1 f.
[29] Beiträge zur Experimentellen Psychologie, Heft 2, 125.
[30] Outlines of Psychology, p. 359.

individual difference in the arrangement of the retinal elements. These hypotheses are respectively considered in paragraphs "7" and "6" below.

(6) *Individual Differences in Field Types.*—The marks of individuality have in large part been mentioned in paragraphs 4 and 5 above. Yet that there are distinctly individual combinations of those variations is shown quite conclusively in series E, Chart I, as well as in the Equal-line series, Chart II. Further individual traits are to be found in the diagonal eccentricities of the graphs of Pe. and Ta., for instance, and in the peculiar outlines of the medial and central field graphs of Py., Chart III. Furthermore, the variant results of other investigators, as above shown, are indicative of marked individuality of *some kind* on the part of the different observers.

Now the fact that in Figure 7 the lines of acuity are not always parallel, or in perfect conformity, is very suggestive of possible irregularities of retinal pattern in the arrangement of the epithelial structures, the rods and cones. And if such irregularities exist why should there not be individual variations in this as in any other feature of the human organism? Why should not individual retinae vary as much in pattern as do the lines of the skin, for instance, that give individuality to a thumb print.[31] There are ways in which all thumb prints are alike, there are ways in which each thumb print must be forever unique. May this not, conceivably, be equally true of the retinae?

It is possible to conceive, also, that momentary causes, or at most *attention habit,* might sufficiently account for many of the phenomena of individual differences. In many instances of differences in results among experimenters this doubtless is a true explanation. Certainly the unstable features of this experiment

[31] Parsons (*Color Vision,* pg. 10) says: "There is physiological evidence that the rod free area [macular region] varies in size in different individuals."
Ruediger (*Archives of Psychology,* No. 5.) finds marked indication of individual variation in the acuity field types. That he does not, however, correlate this phenomenon with existing variations in the amount of the V-H illusion for the same subjects may very possibly be due to the fact that the latter experiments were very brief and made with free rather than constant fixation, which would leave in doubt the retinal areas explored.

—e. g., the practice types of Hu., Chart V—must be accounted for in some such manner. But to ascribe to attention habit those features that persist through all changes—as, for instance, the same subject's overestimation of the right field with respect to the left, of the upper with respect to the lower, and the equally marked characteristics of other subjects—surely this is equivalent to reducing habit to a phylogenetic thing, to structure itself.

Granted that there is a structural basis for innate or fixed habits, might not that structure be in the asymmetries of the ocular muscles? Is there not, without any presumption, a correlation here? Such an inquiry is worthy of analysis. There is unquestionably an inverse correlation between the general arrangement of the ocular muscles and the directions of over- and underestimations in the visual field. Some peculiarity of muscle attachment, for instance, might be thought of as a possible basis for the individual peculiarities in general field outline. But that such an explanation could be extended to meet the variations, or non-conformities, of the interior outlines (e. g., Py., Chart III) is scarcely conceivable. A structural correlation upon a retinal basis seems to the writer far more plausible as an hypothesis.

A further functional fact lending weight to the theory of individual peculiarities in retinal structure is the matter of the color fields. It would indeed be unusual to find two individuals the outlines of whose color charts coincided.—Certainly "muscle strain" is not responsible here.—Why, then, may it not be possible that in other functions, for example space perceptions, the retinae should be responsible for individual differences?

(7) *Attention Data.*—Attention to a line probably means one of two things. Either there may occur the usual involuntary eye shift in the direction of the attention shift, or, conceivably, an increase of neural energy in the marginal retinal parts attending. Either of these conditions would tend to increase the efficiency of the parts brought to bear upon the stimulus. Certainly attention could *not decrease* that efficiency.

Now we have seen in Part III, Sec. 5, of this experiment,

that voluntary attention to a line tended to decrease its subjective extent in comparison with the line left correspondingly in the margin of attention; also, that whenever there was a condition that would tend to attract the involuntary attention toward any particular line, corresponding results followed. If focal attention means, then, in any sense, focal vision, or a tendency toward it, then the data here correlates perfectly with all that is reviewed in the paragraphs above; that is, the greater underestimations are found to accompany the greater acuity, the greater retinal efficiency, and the comparative overestimations, accordingly, are found in the regions of inferior structure and function. This means, a shift of the eye toward a line brings it nearer to the focus and leaves the other line correspondingly toward the margin, or, the change in innervation would mean for the line clearly attended a similar advantage over the lines less attended.

Now that either of the supposed retinal changes positively occurred with changes of the attention in this experiment, cannot positively be demonstrated, since there is yet no X-ray machine for photographing fluctuations in neural activity, and unfortunately no photographs were made of the eye movements during the process of this experimentation. The presumptive evidence in favor of the existence of the eye movements, however, is strong. In the first place, no fact is more general or more generally known, to the common man and the psychologist, than that of the impossibility of *perfect* or unwavering fixation at a given point, or the tendency for the eye to yield to the involuntary or reflex "pull" in the direction of whatever tends at a given instant to draw the attention. Second, the introspections of the subjects were most clear upon this point, especially in the beginning of their work—when their average variations were exceedingly high. Finally, it remains for those who object to the propositions on which these correlations rest, to demonstrate to us—for only the truth is sought—just what does take place with fluctuations of the visual attention.

One such objection again is anticipated, namely, that though "eye shift" should be granted, there is a corresponding change in muscular strain, and that even a controlled *tendency* to eye

shift would mean the greater strain of the "tendency to movement," which Wundt says is as potent in results as movements actually performed. Some facts of the experimental data make this objection at first appear plausible. Part II was, in a sense, a study of marginal attention. It was there found that the V-H illusion increased—or tended to increase—as the stimuli were moved correspondingly nearer and nearer to the margin. If this general V-H disparity be due to an original difference in the strain of the muscles that move the eyes in these two directions, it is conceivable that this difference in strain would increase with the distance traversed, or with the strain of the "tendency" to traverse the given distance. But again in Part III, Sec. 1, there was given an objective field in the *extreme margin* as to actual muscular strain, and there followed no results such as would be required by the muscle-strain theory. It is only when the lines are marginal as to the retina (Part II) that significant changes take place. Aside from this, in the Attention series (Part III, Sec. 5) it was when the horizontal strain—or movement—apparently was greatest—i. e., with the attention on M 0—that the illusion increased, while with attention to the vertical (M 90)—increasing, if anything, the movement or movement tendency in that direction—the illusion decreased. This scarcely seems in correlation with the muscle-strain theory.

We have little knowledge of the minute exactness of the nerve supply of the ocular muscles, such as would enable us to form a conception of what particular combination of their activities would increase or decrease the total strain. We do know a few details of the retinal areas; so much, in fact, that we are sure a movement of one-tenth of an inch shifts the one million cones of the central fovea toward the favored visual object, and a much slighter movement must suffice to shift some hundreds or even thousands of these elements in the given direction. In each instance the less attended line moves correspondingly toward the less favored areas as to structure. The phenomena of focal-peripheral attention (Part III, Sec. 5) apparently are reducible, then, to a correlation with fovea-marginal structure. The presumptive evidence in favor of a retinal explanation for merid-

ional illusions of space is therefore enhanced rather than diminished by the data of the Attention series.

The phenomena of certain of the supplementary tests are most readily accounted for upon the basis of attention attitude, or attention fluctuations, as here explained. See data of "monocular vision," "elevation of the head in primary vision," and "undeveloped mentality," Part I, Sec. 2, series (1), (3) and (5).

The variable data of the experiment as a whole, as well as the more fixed or persistent features of the "field types," are thus found capable of harmonious correlation with retinal structure as it is known from histology and as it is inferred from facts of acuity.

Data from Other Experiments

The psychological principle of Attention, as determined or controlled by fixed retinal structure and by variations in eye position, seems ample, further, to account hypothetically at least for the widely divergent factual data from other investigators cited in preceding pages.

It would be interesting, at least, to know the "attention attitude" of the subjects of Münsterberg and of Stevens, as well as of other authorities whose results are contradictory. Certainly in our right handed reading, writing, and other tasks, we come to attend ordinarily more to the right field than to the left. This might easily lead to the general attention habit that accords with Külpe's statement that "generally" the left field is seen larger than the right.

One of the interesting (and correlating) instances from Lipps may be cited. He states (see page 6) that the mere conception of *any* one of the sides of an equilateral triangle as the *base* leads to an underestimation of that line and a corresponding overestimation of the remaining two. A careful observer will doubtless confirm the statement. But the careful observer will also note that the conceiving of one of the three lines as the base is attended by a *focalization* of the line so conceived. There follows, then, such disparity between the base and sides as correlates perfectly with the usual characteristics of focal and marginal vision, or focal and marginal attention.

Without further detail it may be recalled (see page 29) that in this experiment—with apparently like objective experimental conditions—there were found as widely divergent individual differences as any recorded differences in results of other investigations. If in this case of the uniform conditions it seems possible, may it not be still more probable that in experiments of unlike conditions, the true explanation of the individual variations may rest ultimately upon attention attitude, as controlling and controlled by the relationship between the objective given and the peculiar retinal configuration?

CONCLUSION

If it be asked how the correlations above recited *explain* the meridional disparities in visual space, the answer is, very frankly, that this the writer can no more say (with assurance) than she can state in other than hypothetical terms how the retina transmutes wave lengths into color sensations. The fact of the correlations is the one thing apparently indicated by this study. And somewhat paradoxical these correlations at first may seem. One readily accepts the association of, for instance, two-point acuity with structure; but to correlate relative underestimations of space with areas of clarity of vision, keenness of discrimination, and superiority of neural equipment, and the corresponding overestimations with regions of less efficiency and inferior structure, would scarcely occur to one did not such relations recur again and again in the experimental and other factual data.

But if it has been conceivable to the minds of men that the inverse relation between spacial estimations and muscle structure is meaningful, why should it be a thing impossible to suppose some harmony exists, some meaning resides, in the close relationship between these spacial perceptions and the retina itself? How this relationship came to be or the function it may serve in the economy of nature are questions upon which we may form only what seems to us a reasonable surmise. Mere difference in number of elements—rods and cones—in the macula and the margin of the field, and—because of the shape of the macula—in the vertical and horizontal meridians of the field, *might* in itself be sufficient to account for all disparities. Might it not be that the greater the number of elements brought to bear upon a given object the smaller, the more insignificant, that object would appear, while in the marginal areas of fewer elements mere indistinctness might be translated into a vague bigness? A pencil held between thumb and finger, or by two fingers, is a vaguer object as to size than when grasped by all the fingers at once.

Is there any reason why, in last analysis, *differences in ease of visual attention*—dependent upon relative number or efficiency of elements—may not be sufficient bases for all differences in meridional and foveal-peripheral space?[1]

But turning from the question of how the differences are mediated, one is confronted by the further inquiry of what economy of vision is served by the unequal distribution of structure and function in the different areas of the retina. Is there any need or purpose demanding that the macula should be wider in its upper than in its lower extent, or that it should be four times greater in its transverse than in its vertical diameter?—It seems possible that adaptations to environmental conditions may have required or even occasioned just such an arrangement. The lower objective field, viewed by the upper retina, is the more detailed and critical for life interests, and hence may demand greater clarity of vision, greater ease of attention, finer discrimination, and finally a provision for seeing detailed parts in *smaller compass* than is necessary in apprehending the upper field. The upper objective field is more remote, is less fraught with details of immediate concern, and hence may be thought of as fairly provided for by the less acute vision of the lower retina, if there be not even an aid to far vision in the comparative overestimations of this region. But the panorama of the earth's surface stretches out with its infinite detail in the *horizontal* rather than in the vertical direction. There is less call for attention in the up and down direction, especially up. The seeking of food and the avoidance of danger demand that we attend not only straight forward, immediately before us, but far to the right and to the left, and, further, that there should be minute, clear vision, with *compression* of details into small compass—or comparative underestimations. Here Nature apparently has been lavish in her provision. Not only do the functions and the form of the single macular region contribute to these ends, but we have two eyes, and these are horizontally placed. Again, the

[1] Külpe, fertile in suggestions, says: "It [the V-H illusion] might also be ascribed to the far greater accuracy of judgment (keenness of vision) in the horizontal direction." Outlines of Psychology, p. 365.

peculiar refractive power of the cornea greatly widens vision. And, finally, there is a beautiful adaptation of the ocular muscles[2] to the giving of readiest *attention* in the fields most fraught with sudden emergencies. Thus the monocular field, the binocular field, and the whole field of regard tend to follow harmonious outlines of the oblate oval type.[3] In addition to this we see in the tendency to horizontal compression (underestimation) a further device for extending the visual grasp in that critical dimension of the field. That in a given circular area (such as delimited by this experiment) the vertical meridian should be comparatively overestimated, seems but an accident attending the more important matter of the horizontal compression, the bringing of a wide range of horizontal details into the area of clear vision. Since this is seen (page 76) not to depend upon dioptrical conditions, since there is no evidence that it is automatically connected with eye movements (quite the contrary) and since there is a difference in retinal structure *corresponding* to differences in space estimations, does it not seem reasonable to conclude, until we shall have further light from better investigations, that the retina itself is the ample physical correlate of meridional disparities in the visual field? That part of the brain which "comes out to see" apparently, then, is the chief determinant of the forms that shall be perceived.

One final question arises, namely, what is the bearing of the

[2] May it not be that the true correlation of the asymmetries of the ocular muscles with the facts of vision is found just in this, namely, the extension of the convenient oblate form of the field of vision to the wider outlines of the field of regard? That the direction the eye takes in traversing a line is a cue to hand movements, or other bodily adaptations, is undoubted. But that this is a correlation that must be learned and can be unlearned is evidenced by the young child's inverted script, by the experiences of mirror drawing, and by the more common experiences of the dressing table with its single and double mirrors. That there is a further feeling in the muscles that decides, for example, the visual aspect or form of the figure 8, when common sense is saying, "I *see* it, I image it so," is an unsustained assumption. Neither by common experience nor by experimental evidence does it seem apparent that muscular strain is any more essential to visual space judgments than to "the voluminousness we ascribe to pain," or to any other sensations of a disparate character. (See Külpe's Outlines of Psychology, p. 371, Sec. 5.)

[3] See Figure 4.

data of this experiment upon the theories as to the broader aspects of general space perception? Here again only a modest opinion, not an authoritative response, can be given. It seems to the writer that if the meridional illusions are inherent in retinal structure neither nativist nor empiricist need do violence to his general theory in accounting for these disparities. They come to be in the same way that other space perceptions come—through an interpretation—of *some kind*—of SENSORY data. How far visual space perception approaches an act of *judgment* or of *reason,* how far it may be merely and purely a *sensory given,* is a question for the philosophers. That something akin to judgment enters into our general space interpretation we are led to believe from the fact that the retinal image is smaller than either its objective counterpart or its subjective interpretation. Again, while the area of the whole retinal field stimulated varies little from time to time, one's conception of the space included varies enormously. The image of Niagara is not larger than that of one's own door yard or of the corner of a hall bedroom. If this same corner of a room be decorated with a portrait of Niagara, of a perspective sufficiently hypnotic, it may give the same impression of immensity as that gained from the original of the portrait. What has been termed *judgment* possibly is involved here. A second consideration is that size and distance cues seem to be acquired, not native. A child sees a "colt" from a car window; the father sees in the same object a horse at a distance. The former *attends* solely to the central object of her interest; the latter attends not to that object only, but to the whole field, with its *distance cues* which he has learned through experience. The child's interpretation we term perceptual, the father's judgmental. Yet, in last analysis, what is the act of judgment in this case? Is it not made up solely of a wider and richer *sensory attention?* What has experience given to the father that the child has not yet acquired? Has it not merely increased his power of minute, detailed attention to the data of the whole field? The addition of a new object in the field of view—a human figure at the foot of the Niagara

gorge, a boat or a jut of land in an expanse of sea—immeasurably increases our conception of immensity. What has happened? Nothing except that we have been given a more familiar (sensory) measuring unit. The judgment, as James says of experience, does not make space out of nothingness, it "must be given some grist to grind." The "grist" in this case must be the measuring units, the distance cues, plus the complicating element of variation of retinal structure in the several radii of the field. It is the former that makes up the total aspect or "bigness" of the whole, while the latter determines the variation in outline form, or occasions the apparent meridional disparities of the field; for a given objective "unit" has been found (see Part II, (summary) to have a varying value according as it stimulates one or another of the retinal parts. *Sensory reality,* sensory *attention,* seems, then, the basis of our space judgments, whether of the "normal" or of the so-called "illusory' 'type.

Finally, the only contention of this paper is that it is possible to conceive of the meridional disparities of the visual field as reducible ultimately to retinal structure on the one hand, and to a simple act of visual attention on the other. There is no attempt at disproof, as in Münsterberg's early work, of the existence of the transcendental. The *mind* may perceive—*attend*—as through an open window. But if that window be oval as to structure, the picture must necessarily be oval.

GENERAL DESCRIPTION OF CHARTS AND TABLES

So far as practicable the lettering and other symbols are the same in corresponding charts and tables. The *A*, for instance, in each refers to the first normal series for any subject (or for any series following the normal order of positions for the St. and Var.). The *A′* indicates a later series of the same order (or, if so stated, an *average* of certain features of the A series). In the charts the practice graph, *A′*, is superposed by a broken line, upon the A graph of the earlier data. The same combination for showing practice effects is made in graphs B, C, and D, for subjects Ba., Py., and Ki. The small digits in parentheses, under the graphs, indicate the number of the series upon which the data are based, and thus render evident the amount of practice intervening between any two series represented.

Graphs

The plan of constructing the graphs is as follows: The eight radii are simultaneously represented, reduced to one-tenth their actual length. The over- or underestimation of each is indicated by placing a dot in the extension of the line at a distance from the end (plus or minus) corresponding to one-half the actual amount the Var. (represented by a broken radial line) was lengthened or shortened to give subjective equality in each case. The joining of these dots produces an approximate picture of the subjective "field" of each observer, though the lines for convenience, are straight instead of curved. This further marked divergence in the drawing is also to be noted, namely, that, as indicated by the above scale, the errors of judgment, both positive and negative, are exaggerated to five times, or 500% of, their proportional amounts. All charts are upon the same scale, but in graphs B, C, and D, of Chart I, for subjects Jo. and Pe. a dotted line indicates a reduction of the data to the common basis of M 0, for the greater ease of comparison with the A′ series. (Such reduction of the data was made in all cases to obtain the averages entering into the E series of graphs and tabulation.) In Chart III, the lengths of the two shorter standards, used for comparing the smaller field areas with the "normal", are indicated by heavy dots in the different M's, and the corresponding graphs (*C*, "central"; *M*, "medial") are inserted within the "norm", *N*. Again, in these smaller graphs the over- and underestimations are exaggerated 500%.

Tables

In all tables the first numerical column, "90, 90′, 45, 45′," etc., represents the meridional positions of the standard with relation to the right horizontal radius (M 0), which, in the normal procedure, is the variable. In the column of italicized letters, the *a, b, c,* and *d* represent angular distances (45°, 90°, 135°, and 180°, respectively) of the St. from the actual Var. of the given series. The letters are primed when the distance is below or to the left of the Var. The data column gives the amount in millimeters (unless percentage is indicated) of the exact over- or underestimation (the latter shown by a minus sign) of the given meridian with respect to the variable line of that particular series—or the amount the Var. actually exceeded the length of the St. when the two were pronounced subjectively equal. These results are the averages of a "double series", or a total of twenty judgments, for each meridional position of the St. in each series. The final column, with the figures 1 to 8, given in some of the tabulations, shows the respective *order* of magnitude of the overestimations, or of the apparent length of the respective radii.

TABLE I.—FIELD TYPES. (St., 140 mm.)

Subject:	Series:		A		A′			B			C			D			E	
Jo.	90	*b*	7.9 mm.	5	28.95mm.	2	*b*	20.25mm.	2	*d*	5.1 mm.	2		*Var.*	5½	*b*	15.92mm.	2
	90′	*b′*	12.6	1	38.95	1	*b′*	21.4	1		*Var.*	4	*d*	5.7 mm.	3	*b′*	18.86	1
	45	*a*	8.5	4	24.45	4	*c*	16.74	4	*c*	7.75	1	*a*	0.0	5½	*a*	14.59	3
	45′	*a′*	9.55	3	12.3	6	*c′*	17.85	3	*a*	— .2	5	*c*	5.8	2	*a′*	11.29	6
	135	*c*	7.25	6	23.9	5	*a*	12.1	5	*c′*	3.3	3	*a′*	2.35	4	*c*	12.76	4
	135′	*c′*	12.	2	28.05	3	*a′*	7.1	6	*a′*	— 1.5	6	*c′*	7.5	1	*c′*	12.64	5
	180	*d*	3.35	7	11.5	7		*Var.*	8	*b′*	— 3.3	7	*b′*	— 2.05	7	*d*	3.89	7
	0		*Variable*	8	*Var.*	8	*d*	1.15	7	*b*	— 3.75	8	*b*	— 6.8	8			8
Pe.	90	*b*	23.35mm.	4	24.85mm.	3	*b*	— .35mm.	4	*d*	7.85mm.	2		*Var.*	3	*b*	15.66mm.	2
	90′	*b′*	22.5	5	18.25	5	*b′*	— .75	5		*Var.*	3	*d*	2.55mm.	2	*b′*	14.02	4
	45	*a*	4.85	6	8.3	6	*c*	6.15	2	*c*	— 1.85	4	*a*	— 8.2	7	*a*	8.19	6
	45′	*a′*	3.65	7	8.0	7	*c′*	6.25	1	*a*	— 9.05	7	*c*	— 1.0	5	*a′*	7.91	7
	135	*c*	33.5	1	40.1	1	*a*	— 3.4	6	*c′*	14.35	1	*a′*	— .35	4	*c*	18.97	1
	135′	*c′*	30.8	2	29.9	2	*a′*	— 5.5	7	*a′*	— 5.9	6	*c′*	8.7	1	*c′*	14.97	3
	180	*d*	26.55	3	21.2	4		*Var.*	3	*b′*	— 2.45	5	*b′*	— 2.8	6	*d*	13.27	5
	0		*Var.*	8	*Var.*	8	*d*	— 9.1	8	*b*	—11.5	8	*b*	—11.2	8			8
Hu.	90	*b*	8.4 mm.	1	— 9.1 mm.	6	*b*	— 4.9 mm.	6	*d*	1.25mm.	7		*Var.*	6	*b*	—10.1 mm.	6
	90′	*b′*	— 1.5	7	—15.85	8	*b′*	— 9.95	8		*Var.*	8	*d*	— 3.25mm.	8	b′	—11.65	8
	45	*a*	5.0	3	— 2.6	2	*c*	13.15	1	*c*	18.25	1	*a*	7.1	2	*a*	2.09	1
	45′	*a′*	4.5	4	— 5.65	4	*c′*	9.25	2	*a*	3.4	5	*c*	5.5	3	*a′*	— 3.76	3
	135	*c*	5.3	2	— 4.45	3	*a*	2.1	4	*c′*	9.55	3	*a′*	.25	5	*c*	— 5.02	4
	135′	*c′*	— 1.6	8	— 8.25	5	*a′*	— 8.1	7	*a′*	2.45	6	*c′*	— 1.55	7	*c′*	—10.75	7
	180	*d*	— .3	6	—11.0	7		*Var.*	5	*b′*	7.7	4	*b′*	2.55	4	*d*	— 7.71	5
	0		*Var.*	5	*Var.*	1	*d*	7.3	3	*b*	12.85	2	*b*	7.4	1			2

Ba.	90	*b*	9.42mm.	4	15.7 mm.	1	*b*	14.0 mm.	2	*d*	10.9 mm.	1		*Var.*	5½	*b*	10.06mm.	4
	90′	*b′*	12.75	3	6.55	2	*b′*	11.3	4		*Var.*	4	*d*	7.45mm.	3	*b′*	7.25	6
	45	*a*	3.35	6			*c*	13.8	3	*c*	2.9	2	*a*	0.0	5½	*a*	8.25	5
	45′	*a′*	7.32	5			*c′*	14.75	1	*a*	— .35	5	*c*	10.5	1	*a′*	12.74	2
	135	*c*	21.2	1			*a*	6.35	6	*c′*	.6	3	*a′*	2.	4	*c*	11.22	3
	135′	*c′*	20.17	2			*a′*	9.3	5	*a′*	— .9	6	*c′*	9.3	2	*c′*	14.16	1
	180	*d*	3.67	7	2.15	3		*Var.*	7	*b′*	— 7.75	7	*b′*	— 8.75	8	*d*	.19	7
	0		*Var.*	8	*Var.*	4	*d*	— .45	8	*b*	— 9.9	8	*b*	— 8.4	7			8
Py.	90	*b*	15.07mm.	2	8.5 mm.	1	*b*	7.7 mm.	4	*d*	2.6 mm.	3		*Var.*	7	*b*	7.95	5
	90′	*b′*	11.17	6	2.2	3	*b′*	9.17	3		*Var.*	6	*d*	6.97mm.	2	*b′*	8.65	4
	45	*a*	12.8	4			*c*	7.7	5	*c*	— 1.72	7	*a*	1.57	6	*a*	6.77	6
	45′	*a′*	12.55	5			*c′*	10.9	2	*a*	4.37	2	*c*	7.95	1	*a′*	11.02	1
	135	*c*	16.3	1			*a*	3.77	6	*c′*	6.85	1	*a′*	2.75	4	*c*	9.59	3
	135′	*c′*	13.47	3			*a′*	12.52	1	*a′*	1.72	4	*c′*	6.75	3	*c′*	10.79	2
	180	*d*	9.6	7	4.15	2		*Var.*	8	*b′*	.9	5	*b′*	1.77	5	*d*	2.96	7
	0		*Var.*	8	*Var.*	4	*d*	.07	7	*b*	— 5.37	8	*b*	— 3.42	8			8
Ki.	90	*b*	14.17mm.	2	14.1 mm.	1	*b*	2.8 mm.	4	*d*	5.0 mm.	1		*Var.*	1	*b*	7.96mm.	1
	90′	*b′*	3.27	6	3.65	2	*b′*	5.92	2		*Var.*	2	*d*	— 7.87mm.	7	*b′*	3.27	2
	45	*a*	6.25	5			*c*	6.55	1	*c*	— 5.72	8	*a*	— 6.35	5	*a*	2.62	4
	45′	*a′*	— 3.07	8			*c′*	4.67	3	*a*	— 4.45	5	*c*	— 8.57	8	*a′*	— .42	8
	135	*c*	11.02	3			*a*	— 2.45	7	*c′*	— 4.17	4	*a′*	— 3.6	3	*c*	2.63	3
	135′	*c′*	15.92	1			*a′*	— 2.97	8	*a′*	— 5.45	6	*c′*	— 2.1	2	*c′*	1.29	5
	180	*d*	10.82	4	2.35	3		*Var.*	6	*b′*	— 3.12	3	*b′*	— 6.3	4	*d*	.72	6
	0		*Var.*	7	*Var.*	4	*d*	2.42	5	*b*	— 5.57	7	*b*	— 6.6	6			7
Ca.	90	*b*	17.45mm.	1	13.95mm.		*b*	11.45mm.	2	*d*	10.3 mm.	1		*Var.*	2	*b*	8.74mm.	1
	90′	*b′*	13.6	2			*b′*	7.35	6		*Var.*	5	*d*	— 1.45mm.	4	*b′*	3.81	5
	45	*a*	8.35	6			*c*	11.9	1	*c*	7.0	3	*a*	— 2.65	5	*a*	5.34	3
	45′	*a′*	11.7	3			*c′*	10.7	3	*a*	.25	4	*c*	7.8	1	*a′*	6.55	2
	135	*c*	8.6	5			*a*	7.85	5	*c′*	8.3	2	*a′*	— 2.8	6	*c*	4.42	4
	135′	*c′*	7.15	7			*a′*	6.2	7	*a′*	— .15	6	*c′*	— .85	3	*c′*	2.02	6
	180	*d*	10.25	4				*Var.*	8	*b′*	— 3.95	8	*b′*	— 9.65	8	*d*	— 1.9	8
	0		*Var.*	8			*d*	10.5	4	*b*	— 1.95	7	*b*	— 4.3	7			7

TABLE II. EQUAL LINE SERIES.

A. COMPARISON OF OTHER MERIDIANS WITH RIGHT HORIZONTAL.

Subject:	Hu.		Ta.		Jo.		Be.	
90	9.45 mm.	1	3.1 mm.	5	1.4 mm.	5	15.2 mm.	1
90′	—4.64	8	4.5	4	9.6	1	13.5	3
45	—2.6	6	2.5	6	1.2	6	.8	7
45′	3.85	2	1.3	7	4.0	3	10.3	5
135	2.15	3	5.9	2	1.5	4	13.1	4
135′	—1.9	5	7.6	1	9.4	2	14.5	2
180	—3.15	7	5.4	3	.4	7	9.0	6
0		4		8		8		8

B. COMPARISON OF OPPOSITE PAIRS OF EQUAL LINES.

Subject:	Hu.	Ta.	Jo.	Be.	Pe.
Upper vs. Lower Vert.	7.4 mm.	— .6 mm.	.3 mm.	.2 mm.	1.8 mm.
Right vs. Left Horiz.	3.6	—5.1	.3	—10.7	—6.0
Rt. Up. vs. Lft. Lo. Diag.	2.5	—4.7	—3.2	—11.8	—3.5
Lft. Up. vs. Rt. Lo. Diag.	2.	5.6	— .5	10.6	4.0

C. COMPARISON OF EIGHT MERIDIANS SIMULTANEOUSLY EXPOSED.

Subject:	Hu.		Jo.		Pe.		Ca.		Ki.		Ki. (C′)	
90		8	8.5 mm.	1	3.0 mm.	5½	3.0 mm.	1+	6.2 mm.	1	5.5 mm.	1
90′	5.0 mm.	3	6.5	2	3.3	4	3.0	1+	1.3	6	1.3	6
45	4.0	4	6.0	3	1.5	7	2.0	4+	2.0	4½	2.0	4½
45′	3.0	6	3.0	7	3.0	5½	1.5	7	2.0	4½	2.0	4½
135	3.5	5	4.5		4.0	2	2.0	4+	5.0	2	5.0	2
135′	1.0	7	4.5		3.5	3	3.0	1+	.1	7	.1	7
180	8.5	1	4.5		7.0	1	2.0	4+	3.0	3	3.0	3
0	8.0	2		8		8		8		8		8

TABLE III. Part 1.—LENGTH OF STANDARDS VARIED

Series A. (Var. at M o.)— Absolute Values.

Subjects:	Ba.			Py.			Ki.			Ca.		
	N	M	C	N	M	C	N	M	C	N	M	C
St. length:	140 mm.	85 mm.	30 mm.	140 mm.	85 mm.	30 mm.	140 mm.	85 mm.	30 mm.	140 mm.	85 mm.	30 mm.
90	15.7 mm.	8.3 mm.	2.0 mm.	12.4 mm.	—2.1 mm.	—2.8 mm.	14.1 mm.	4.1 mm.	.7 mm.	17.5 mm.	11.2 mm.	5.8 mm.
90′	6.5	7.2	2.5	8.6	.7	.2	3.7	1.9	.45	13.6		
45	3.3	3.1	1.5	9.5	1.5	—2.1	2.0	2.5	.5			
45′	7.3	7.4	1.9	10.	4.0	—1.5	— 1.2	3.1	.9			
135	18.2	15.0	5.0	11.	2.5	—1.3	5.4	4.4	1.2			
135′	16.1	16.1	5.5	11.2	—.4	.1	8.1	3.2	.7			
180	2.2	4.9	.4	6.4	.1	—2.6	5.6	1.7	2.3	10.3		
0												

Relative Values.

Subjects:	Ba.			Py.			Ki.			Ca.		
	N	M	C	N	M	C	N	M	C	N	M	C
St. length:	140 mm.	85 mm.	30 mm.	140 mm.	85 mm.	30 mm.	140 mm.	85 mm.	30 mm.	140 mm.	85 mm.	30 mm.
90	11.2%	9.8%	6.7%	8.9%	—2.4%	—9.2%	10. %	4.8%	2.3%	12.5%	13.2%	19.3%
90′	4.7	8.4	8.3	6.1	.8	.7	2.6	2.2	1.5	9.7		
45	2.3	3.6	5.	6.7	1.7	—7.	1.4	2.9	1.6			
45′	5.2	8.7	6.3	7.1	4.7	—5.	— .8	3.6	3.			
135	13.	17.6	16.7	7.8	2.9	—4.3	3.8	5.2	4.			
135′	11.5	19.	18.3	8.0	— .5	.3	5.8	3.7	2.3			
180	1.5	5.8	1.2	4.6	.1	—8.5	4.0	1.9	7.7	7.3		
0												

Series E. (Average of 4 Positions of the Var.)—Absolute Values.

Subjects:	Ba.			Py.			Ki.			Ca.		
	N	M	C	N	M	C	N	M	C	N	M	C
St. length:	140 mm.	85 mm.	30 mm.	140 mm.	85 mm.	30 mm.	140 mm.	85 mm.	30 mm.	140 mm.	85 mm.	30 mm.
90	11.2 mm.	6.4 mm.	1.12 mm.	8.2 mm.	— .2 mm.	— .7 mm.	7.9 mm.	2.5 mm.	—1.9 mm.	11.2 mm.	5.9 mm.	1.5 mm.
90′	5.1	3.6	1.15	6.8	1.7	.1	3.3	— .4	—1.8	3.8		
180	.6	.2	— .4	2.5	—1.2	— .6	.7	— .9	—1.	— 1.9		

Relative Values.

Subjects:	Ba.			Py.			Ki.			Ca.		
	N	M	C	N	M	C	N	M	C	N	M	C
St. length:	140 mm.	85 mm.	30 mm.	140 mm.	85 mm.	30 mm.	140 mm.	85 mm.	30 mm.	140 mm.	85 mm.	30 mm.
90	7.9%	7.5%	3.7%	5.8%	— .23%	—2.3%	5.7%	3. %	—6.3%	7.9%	6.9%	4.9%
90′	3.6	4.3	3.8	4.8	2.	.2	2.3	— .5	—6.	2.7		
180	.4	.3	—1.3	1.8	—1.4	—2.1	.5	—1.1	—3.3	—1.35		

Subject:	Ba.				Py.			
	St. 1.	St. 1-D. 1.	St. 1-D. 2.	St. 1.-D. 3.	St. 1.	St. 1-D. 1.	St. 1-D. 2.	St. 1.-D. 3.
0	.....	......	....	......	....	....	....	....
90	7.8%	9.7%	11.0 %	10.2%	3.6 %	3.2 %	5.8 %	8.5
90′	.4.1	4.9	3.46	1.1	3.3	2.2	3.9	5.6
180	1.2	1.8	2.18	2.4	.57	.03	1.03	1.8
	St. 2.	St. 2-D. 1.	St. 2-D. 2.		St. 2.	St. 2-D. 1.	St. 2-D. 2.	
0	.....	......	......		....	....	....	
90	7.4%	10.5%	12.5%		5.92%	7.58%	2.5 %	
90′	4.9	6.4	7.4		2.87	.83	1.5	
180	1.2	1.4	3.2		.02	— .46	.48	
	St. 3.	St. 3-D. 1.			St. 3.	St. 3-D. 1.		
0	......	.....			....	....		
90	4.46%	14.4%			.73%	—3.5%		
90′	— .43	2.8			4.73	—1.6		
180	—5.01	— 3.7			—5.57	—2.5		

TABLE III, Part 3, *a*.

Subject: Ba.

Series		St. 1 (140)	S. 1 (7.5)	S. 2 (22.5)	S. 3 (55)	S. 4 (55)
A	0	....				
	90	11.21%	4.66%	7.33%	7.73%	17.45%
	90′	4.68	—14.	15.77	20.58	18.36
	180	1.53	—15.3	6.66	8.09	6.45
A′	0	....				
	90	13.2 %	—31.33%	13.1 %	14.9 %	16.8 %
	90′	8.25	.44	— 5.4	8.06	4.1
	180	5.42	—10.44	— 9.63	9.2	5.86
E′	0	....				
	90	7.8 %	—25.4 %	14.4 %	12.5 %	10.2 %
	90′	4.1	—10.2	2.8	7.4	1.1
	180	1.2	—10.2	— 3.7	3.2	2.4

Part 3, *b*.

		St. 1 (140)	S. 1 (7.5)	S. 2 (22.5)	S. 3 (55)	S. 4 (55)	*Sum of parts (140)*
A	90	*15.7 mm.* *11.21%*	.35mm. .25%	1.65mm. 1.17%	4.25mm. 3.03%	9.6 mm. 6.8 %	*15.85mm.* *11.25%*
A′	90	*18.53mm.* *13.2 %*	—2.35mm. —1.68%	2.96mm. 2.11%	8.2 mm. 5.85%	9.27mm. 6.62%	*18.08mm.* *12.9 %*
E′	90	*11.04mm.* *7.8 %*	—1.91mm. —1.36%	3.25mm. 2.3 %	6.9 mm. 4.92%	5.65mm. 4.03%	*13.89mm.* *9.89%*
	90′	*5.79mm.* *4.13%*	— .77mm. — .55%	.64mm. .46%	3.99mm. 2.85%	.64mm. .46%	*4.5 mm.* *3.22%*
	180	*1.68mm.* *1.2 %*	.77mm. .55%	— .84mm. — .6 %	1.83mm. 1.3 %	1.33mm. .95%	*3.09mm.* *2.2 %*

Series E'.

Ki. St. 1.	Ki. St. 1-D. 1.	Ki. St. 1-D. 2.	Ki. St. 1.-D. 3.	Ca. St. 1.	Ca. St. 1-D. 1.	Ca. St. 1-D. 2.	Ca. St. 1.-D. 3.
4.73%	5.9 %	8.45%	6.47%	6.51%	5.7%	8.9%	8.82%
—1.43	— .44	.33	—1.67				
—1.07	— .5	.57	1.47				
St. 2.	St. 2-D. 1.	St. 2-D. 2.		St. 2.	St. 2-D. 1.	St. 2-D. 2.	
......							
3.55%	5.57%	8.83%		8.8%	7.8%	5.62%	
—1.35	.35	2.38					
—1.48	— .58	— .83					
St. 3.	St. 3-D. 1.			St. 3.	St. 3-D. 1.		
......							
—2.95%	—1.38%			9.0%	5.7%		
—7.3	—3.15						
—2.03	.87						

Subject: Py.

		St. 1 (140)	S. 1 (7.5)	S. 2 (22.5)	S. 3 (55)	S. 4 (55)	Sum of parts (140)
				a.			
A	0	......					
	90	8.85%	—33.6 %	— 1.92%	1.22%	11.45%	
	90'	6.12	—11.3	4.66	1.63	8.0	
	180	4.58	—26.6	— 2.44	4.9	9.36	
A'	0	......					
	90	4.07	— 6.	— 6.8	.07	14.6	
	90'	2.47	—29.11	— 4.37	—2.61	.07	
	180	2.15	30.22	—14.96	.91	2.74	
E'	0	......					
	90	3.6	9.8	— 3.5	2.5	8.5	
	90'	3.3	23.8	— 1.6	1.5	5.6	
	180	.57	24.4	— 2.5	.48	1.8	
				b.			
E'	90	*5.06mm.*	.74mm.	— .79mm.	1.39mm.	4.68mm.	*6.02mm.*
	90	*3.61%*	.53%	— .56%	.99%	3.34%	*4.3 %*

Subject: Ki.

		St. 1 (140)	S. 1 (7.5)	S. 2 (22.5)	S. 3 (55)	S. 4 (55)	Sum of parts (140)
				a.			
A	0	....					
	90	8.75%	2.0 %	2.44%	11. %	12.54%	
	90'	2.1	8.0	— .66	6.72	10.84	
	180	— 1.96	6.0	9.33	6.36	11.45	
A'	0	....					
	90	11.73	—13.3	2.5	10.1	16.5	
	90'	4.85	— 1.33	—7.7	4.57	10.34	
	180	4.39	—15.77	1.78	.64	6.	
E'	0	....					
	90	4.73	—15.86	—1.38	8.83	6.46	
	90'	— 1.43	—18.9	—3.15	2.38	— 1.67	
	180	— 1.07	—10.8	.87	— .82	1.47	
				b.			
E'	90	*6.62mm.*	— 1.19mm.	—3.1 mm.	4.86mm.	3.56mm.	*4.13mm.*
	90	*4.73%*	— .85%	—2.21%	3.47%	2.54%	*2.95%*

Subject: Ca.

		St. 1 (140)	S. 1 (7.5)	S. 2 (22.5)	S. 3 (55)	S. 4 (55)	Sum of parts (140)
				a.			
A	90	12.46%	39.3 %	14.66%	10.17%	3.63%	
A'	90	11.73	7.7	8.8	10.1	11.6	
E'	90	6.51	13.86	5.7	9.8	8.82	
				b.			
E'	90	*9.12mm.*	1.04mm.	1.3 mm.	3.09mm.	4.85mm.	*10.28mm.*
	90	*6.51%*	.74%	.93%	2.2 %	3.46%	*7.33%*

TABLE IV. OCULAR POSITION, OR POSITION OF EYES IN SOCKETS VARIED.

	Normal		Looking Down 30°		Looking Up 30°		Looking Left 30°		Normal		Looking Right 30°		Normal	
Hu.														
90	8.85	4	7.2	4	2.7	4	—6.2	7	—1.65	5	— 2.13	5	— 5.82	6
90′	1.95	7	.57	7	— .4	7	—4.37	6	—4.82	7	—10.55	8	— 8.02	7
45	9.8	3	9.15	3	5.47	1	— .32	4	2.	2	1.75	2	.65	1
45′	6.7	5	3.77	6	2.2	5	.75	1	—2.02	6	— 5.85	7	— 3.6	4
135	16.1	1	11.9	1	3.07	3	.1	2	2.05	1	4.17	1	— 3.7	5
135′	13.65	2	10.47	2	3.27	2	— .35	5	1.22	3	— .02	4	— 3.42	3
180	4.7	6	5.	5	—1.9	8	—8.9	8	—4.9	8	— 2.92	6	—10.1	8
0		8		8		6		3		4		3		2
Ta.														
90	42.22	3	57.9	4	59.65	4	56.75	4	65.0	4	64.84	4	68.95	4
90′	35.65	5	50.	5	55.2	5	52.52	5	56.67	5	47.35	5	57.75	5
45	14.35	6	17.02	6	28.82	6	18.65	7	21.15	6	23.12	6	17.37	6
45′	10.7	7	16.5	7	19.8	7	18.95	6	19.05	7	11.02	7	14.22	7
135	58.55	1	80.72	1	94.52	1	88.55	1	92.55	1	92.1	1	88.72	1
135′	51.52	2	71.87	2	86.	2	71.5	2	72.02	2	84.77	2	78.05	2
180	41.9	4	61.12	3	70.	3	65.12	3	70.07	3	76.17	3	64.75	3
0		8		8		8		8		8		8		8
Pe.														
90	22.68	4					17.35	5	22.75		24.5	4	32.75	
90′	20.52	5					26.42	4	24.85		16.2	5	37.45	
45	13.47	6					12.0	6			10.25	6		
45′	6.91	7					7.85	7			7.15	7		
135	36.92	1					26.97	3	33.5		32.6	1		
135′	26.87	2					34.97	1	30.35		31.7	2		
180	24.92	3					33.8	2			27.8	3		
0		8						8				8		
Jo.														
90	9.9	6	26.92	3	21.47	3	25.47	3	28.65	3	22.25	3	23.2	3
90′	19.17	2	27.35	2	30.57	1	27.4	2	32.67	1	28.95	1	32.4	1
45	11.45	5	18.5	5	15.1	5	21.37	5	21.0	5	14.22	6	20.95	4
45′	11.5	4	16.2	6	15.22	4	12.72	7	17.9	7	15.57	5	14.9	6
135	12.	3	21.82	4	15.02	6	24.67	4	26.92	4	20.45	4	19.9	5
135′	20.62	1	29.55	1	28.42	2	30.35	1	29.62	2	26.35	2	28.45	2
180	4.3	7	13.55	7	13.75	7	20.66	6	18.	6	11.15	7	13.65	7
0		8		8		8		8		8		8		8

TABLE V. BODILY POSITION, OR POSITION OF HEAD VARIED

Subject Hu.

	Normal		Reclining Left		Reclining Right		Head Inverted	
90	—12.4	7	—12.36	6	—11.66	6	—17.05	7
90′	—11.35	6	—12.76	7	—14.6	8	—19.6	8
45	— 5.65	3	— .12	2	1.59	1	— 4.5	2
45′	— 3.85	2	— 3.21	3	— 5.8	3	—12.45	4
135	— 5.87	4	— 5.2	4	— 8.7	4	—12.1	3
135′	— 7.1	5	—11.2	5	— 9.2	5	—15.85	5
180	—12.5	8	—12.9	8	—12.3	7	—16.75	6
0		1		1		2		1
			Pe.					
90′	20.52	5	21.	5	21.54	5	13.15	4
90	22.68	4	21.04	4	13.02	4	11.65	5
45	13.47	6	9.25	6	3.06	7	— .95	7
45′	6.91	7	2.61	7	4.78	6	— 2.05	8
135	36.92	1	32.	2	25.2	1	34.95	1
135′	26.87	2	32.14	1	22.55	3	25.2	2
180	24.92	3	22.94	3	23.5	2	22.3	3
0		8		8		8		6
			Jo.					
90	23.2	3	29.02	1	20.91	2	21.25	4
90′	32.4	1	26.44	3	23.2	1	27.4	1
45	20.95	4	17.61	5	10.75	7	24.	3
45′	14.9	6	14.51	7	12.2	6	14.7	6
135	19.9	5	17.34	6	20.5	3	20.3	5
135′	28.45	2	26.7	2	20.45	4	24.8	2
180	13.65	7	20.8	4	12.61	5	13.5	7
0		8		8		8		8

TABLE VI. TYPICAL PRACTICE STAGES.

WITH ALSO TYPICAL RESULTS OF THE "IN" AND "OUT" ADJUSTMENTS OF THE VARIABLE.

(Overestimations in mm. of the Vertical with respect to the Horizontal line.)

Series:		1st.		4th.		27th.	
		mm.	mm.	mm.	mm.	mm.	mm.
Ta.	O I	1.2±2.64 17.3±3.24	(9.25 Av.)	26.0±2. 24.3± .9	(25.15 Av.)	63.6±1.52 63.5± .4	(63.55 Av.)
		4th.		10th.		18th.	
Hu.	O I	4.4±4.7 9.8±2.24	(7.1 Av.)	11.4±1.12 9.6±2.08	(10.5 Av.)	9.9± .92 9.7±1.44	(9.8 Av.)
		22nd.		24th.		39th.	
Hu.	O I	—7.0± .8 —3.8±2.16	(—5.4 Av.)	— 2.5±1.5 — 2.2±1.65	(—2.35 Av.)	—7.2± .85 —7.5±2.25	(—7.35 Av.)
		1st Nor.		17th.		48th.	
Jo.	O I	6.2±1.52 8.4±1.28	(7.3 Av.)	26.8±2.56 25.3±1.44	(26.05 Av.)	24.3±2.84 25.0±1.	(24.65 Av.)
		1st Nor.		19th.		30th.	
Pe.	O I	10.4±1.1 36.3±2.2	(23.35 Av.)	25.1±1.68 21.3±1.75	(23.2 Av.)	16.2± .77 16.3±1.0	(16.25 Av.)
		1st.		58th.			
Ba.	O I	4.8±1.84 20. ±2.	(12.4 Av.)	17.0±2.5 16.5±1.0	(16.75 Av.)		
		1st.		61st.			
Py.	O I	4.7±4.76 15.2±1.84	(9.95 Av.)	4.5±1.6 5.3±1.04	(4.9 Av.)		
		1st Nor.		6th.		38th.	
Ki.	O I	14.1±1.72 22.0±2.8	(18.05 Av.)	10.6±3.32 10.0±4.8	(10.3 Av.)	17.8±2.16 14.3±2.36	(16.05 Av.)
		1st Nor.		2nd (Var. left)		14th.	
Ca.	O I	10.6±2.7 24.3±4.4	(17.45 Av.)	13.2±3.3 9.7±2.16	(11.45 Av.)	15.4±1.36 14.3± .96	(14.85 Av.)

TABLE VII. THE DIRECTION OF THE ATTENTION ALTERED.

(St. = M 90; Var. = M 0.)

Subject:	Hu.		Pe.		Jo.	
Attention on:	St.	Var.	St.	Var.	St.	Var.
90	—10.05mm.	3.2 mm.	18.85mm.	25.8 mm.	21.15mm.	20.8 mm.
90′	—21.15	2.15	12.9	19.45	17.1	19.75
45	.35	1.85	11.35	8.75	15.9	18.15
45′	— 6.4	—3.45	1.2	1.0	11.35	11.15
135	— 1.75	2.05	27.1	26.55	20.05	21.0
135′	—13.5	—4.8	21.0	24.55	23.8	24.5
180	— 8.2	—7.55	20.25	18.95	14.15	16.6

CHART I. Field Types

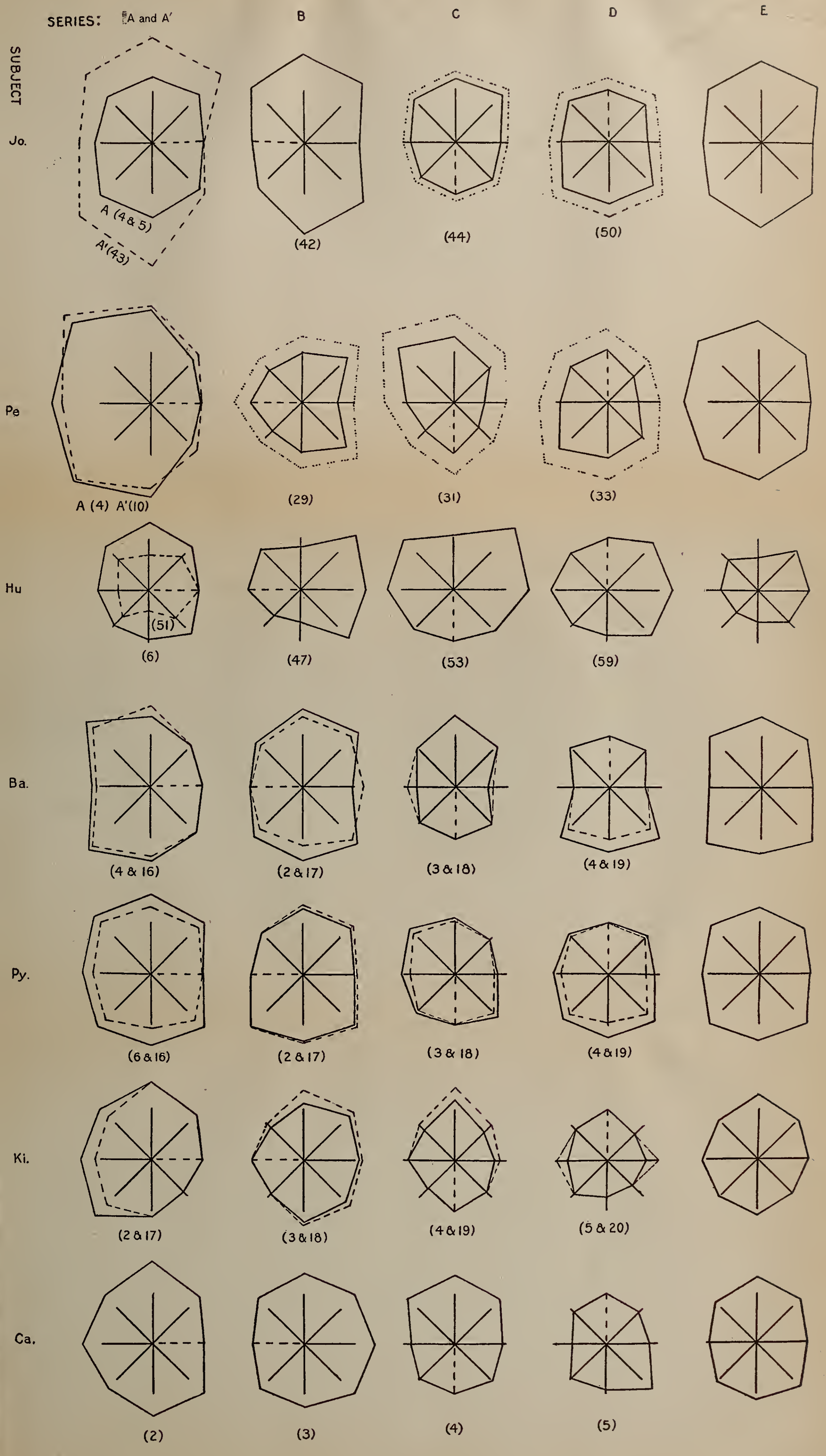


CHART II. Equal Line Series.

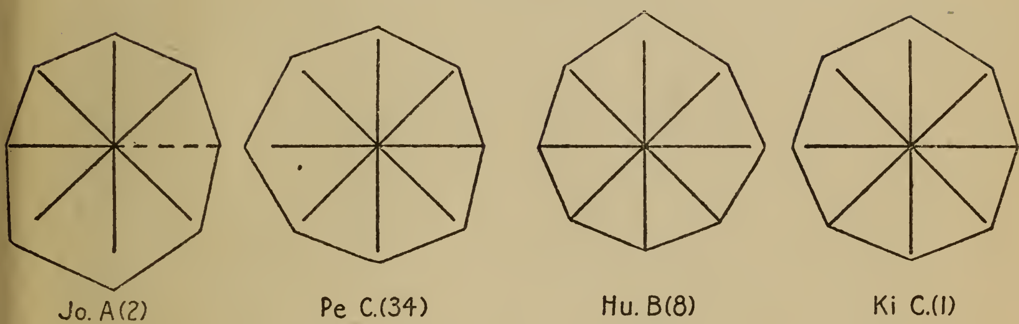


CHART III. Length of Standards Varied.

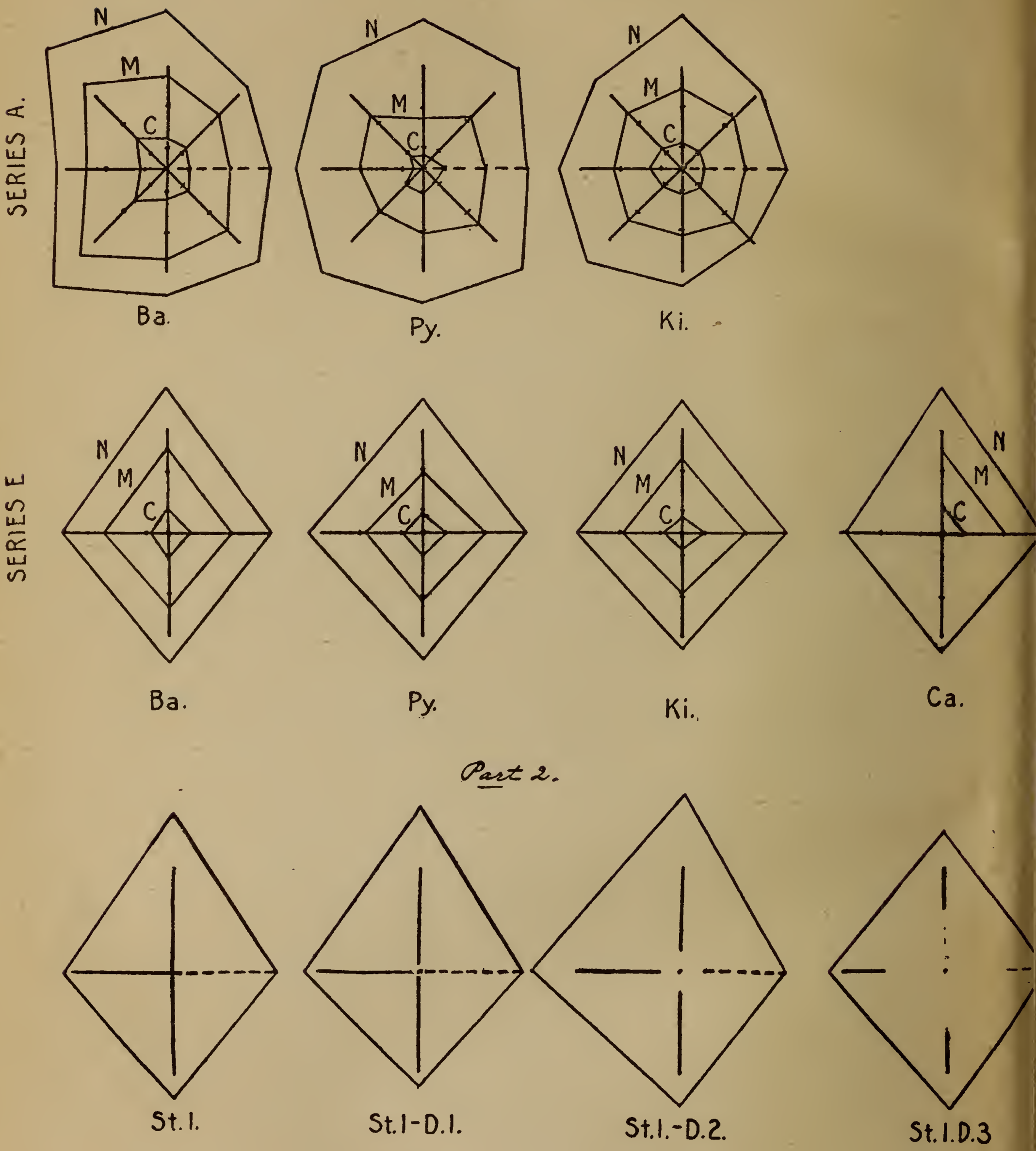


CHART IV. Bodily Position Varied.

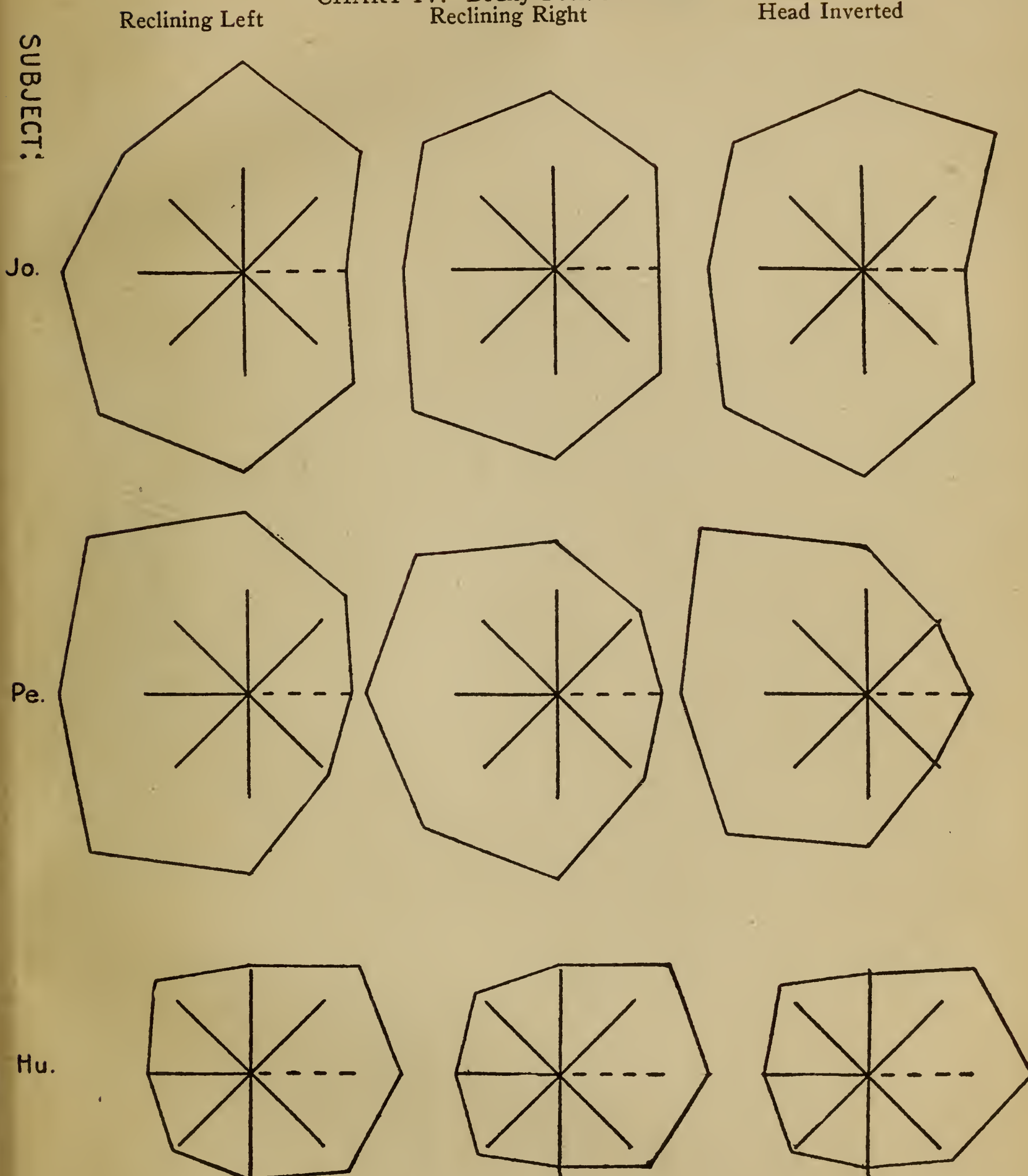
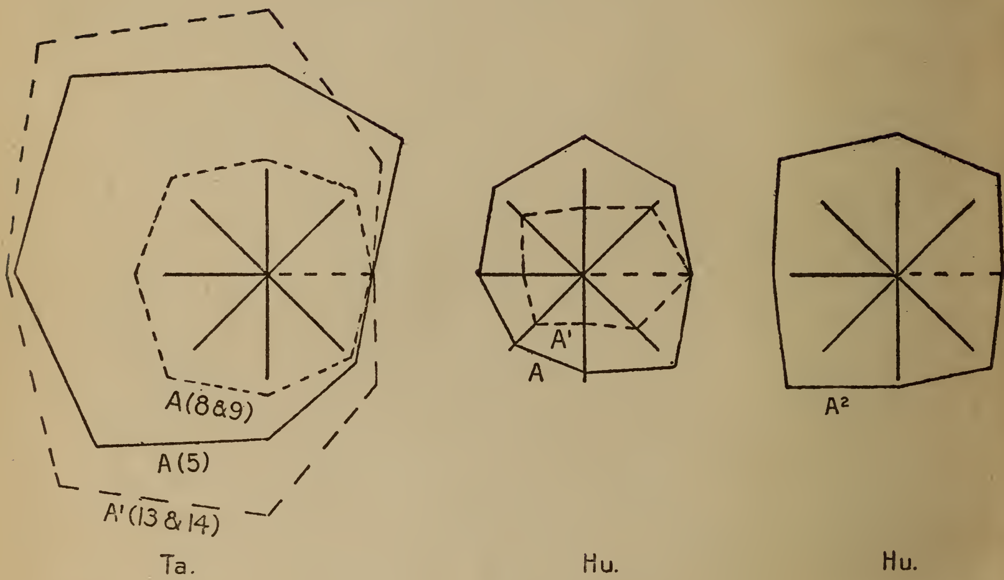


CHART V. Practice Effects



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